Distributive Justice Development: Cross-cultural, Contextual, and Longitudinal Evaluations

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ENRIGHT, ROBERT D.; BJERSTEDT, ÅKE; ENRIGHT, WILLIAM F.; LEVY, VICTOR M., JR.; LAPSLEY, DANIEL K.; BUSS, RAY R.; HARWELL, MICHAEL; and ZINDLER, MONICA. Distributive Justice Development: Cross-cultural, Contextual, and Longitudinal Evaluations. CHILD DEVELOPMENT, 1984, 55, 1737–1751. The development of distributive justice was examined with the Distributive Justice Scale (DJS) in 3 studies. In Study 1, 176 children, ages 7, 9, and 11, from Sweden and the United States were given the DJS and 2 Piagetian logical reasoning tasks. Significant age trends in DJS scores and the relation with logical reasoning were comparable in the 2 cultures. In Study 2, 75 5- and 7-year-old children were given the standard peer DJS and a comparable family DJS to assess reasoning in different contexts. Family stimuli elicited higher levels of reasoning than peer stimuli. In Study 3, 84 6- and 9-year-old children were administered the DJS twice at 1-year intervals. Age trends with no cohort biases were found. Implications for distributive justice research are drawn.

Distributive justice development describes a child’s progressive understanding of what constitutes fair criteria for the distribution of goods. This positive justice domain was first recognized by Damon (1975), and it has attracted much empirical interest in recent years. It is hypothesized that distributive justice follows an orderly stage sequence. In the first stage, 0-A, the child believes that whoever wants the most money or goods should have it. In stage 0-B, the child is said to base distributive decisions solely on external characteristics. For example, the oldest or tallest child should get more than the other children. In the next stage, 1-A, the child believes that everyone should receive the same amount, regardless of qualifying characteristics. In stage 1-B, the child rewards effort and bases decisions on behavioral reciprocity. In other words, the child believes that those who work harder or do more should receive more. In stage 2-A, the child distributes according to

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[Child Development, 1984, 55, 1737–1751. © 1984 by the Society for Research in Child Development, Inc. All rights reserved. 0009-3920/84/5505-0011$01.00]
age trends, a significant relationship with Piagetian reciprocity tasks, a minimal relationship with verbal ability, and a replication in an African culture. The internal consistency reliability has been shown to be adequate for the range of ages reported in this and all subsequent studies (Enright et al., 1980; Enright, Note 2).

For the logical reasoning tasks, the classic Piagetian tasks of liquid and mass conservation (Flavell, 1963) were given to each child. Liquid conservation involves pouring water from one size beaker into another size, thus transforming shape, but not quantity. The child must decide which beaker has more liquid. Strict scoring on a pass-fail dimension was used; the child had to clearly state that liquid quantity was conserved in the liquid transfer. Mass conservation involves transforming one of two ball-shaped masses of clay into an oblong-shaped mass. Again, strict pass-fail criteria were used for the child’s responses to which mass has more clay. Both were given so that the logical and social reciprocity relation, if found with one Piagetian task, could be replicated with the other.

Procedure.—For the purposes of this study, the DJS was translated into Swedish. The original pictures were judged appropriate by a Swedish professional in developmental psychology and hence retained. Three Swedish examiners were trained using detailed administration instructions (see Enright, Note 2). All examiners practiced the procedure first with adults and then with children (two at each appropriate grade level) before beginning the study. Within the DJS the order of dilemmas was randomized, as were the liquid and mass components of the logical reasoning task. The order of presentation of the DJS and logical reasoning tasks was also randomized. For the American subjects, a similar procedure using the standard DJS and logical reasoning tasks was used.

Results and discussion.—Internal consistency was adequate. The percentage of time American children matched stage on the two DJS dilemmas was 65% (significantly different from chance at p < .05 by the binomial z statistic). For the Swedish children, the internal consistency was 51%, also significant, p < .05.

Distributive justice development was examined in a 3 (age) × 2 (country) × 2 (gender) ANOVA design. The main effects for age, F(2,164) = 12.00, p < .001, and for gender, F(1,164) = 6.60, p < .05, were both significant. The important variable for country was not significant; both groups of children developed in a similar way. There were no significant interactions. To further examine the age effects, we used post-hoc Scheffé contrasts at p < .05 collapsed across countries because no interactions between grade and country were found. The analysis revealed that the 7-year-olds were significantly different from the 11-year-olds, and the 9-year-olds were also significantly different from the 11-year-olds. Means and standard deviations are in Table 1.

The next analysis examined the relation between distributive justice reciprocity and logical reciprocity. A point-biserial correlation could not be performed here, since the great majority of sixth graders had passed both conservation tasks, leaving no variability in these scores. To eliminate any ceiling biases and to test the hypothesis that Piagetian logical reciprocity precedes social cognitive reciprocity, contingency tables such as Table 2 were constructed. Logical reciprocity is defined as the child’s passing a test of conservation, whereas social reciprocity is defined as the child’s distributive justice total score being 1.5 or higher. Only at 1.5 does the subject child begin to compensate individuals for special characteristics they have, thus showing reciprocity for those characteristics. Table 2 shows the presence or absence of both logical and social reciprocity across age. The relation is shown with both liquid and mass conservation. The two logical domains were not collapsed to allow for a direct comparison with similar data for younger children in Enright et al. (1980).

A previous study (Enright et al., 1980) had shown that as children develop from kindergarten through fourth grade, they progress from the lower right cell (showing neither kind of reciprocity) in kindergarten, to the lower left cell (showing logical reciprocity only) in second grade, and the upper left (both reciprocities) in the oldest group. Few, as expected, progressed to the “error” cell in the upper right, where social but not logical reciprocity is realized. When this happened, it tended to occur in the youngest group. As Table 2 shows, the data for both liquid and mass conservation in both countries suggest the expected trend. The 7-year-olds are primarily in the lower left cell, and the 11-year-olds are primarily in the upper left cell.

To test this, multinomial analyses were performed on the dichotomous responses across age for liquid conservation and distributive justice within each country. First, in the American sample, the shift from the pass logical and fail social reciprocity cell to the
### TABLE 1

MEANS AND STANDARD DEVIATIONS FOR THE DISTRIBUTIVE JUSTICE SCALE IN THE UNITED STATES AND SWEDEN

<table>
<thead>
<tr>
<th>Ages</th>
<th>United States</th>
<th>Sweden</th>
<th>Countries Collapsed</th>
</tr>
</thead>
<tbody>
<tr>
<td>7 years:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \bar{X} )</td>
<td>1.33</td>
<td>1.41</td>
<td>1.37</td>
</tr>
<tr>
<td>SD</td>
<td>.46</td>
<td>.44</td>
<td>.45</td>
</tr>
<tr>
<td>N</td>
<td>14</td>
<td>14</td>
<td>28</td>
</tr>
<tr>
<td>9 years:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \bar{X} )</td>
<td>1.67</td>
<td>1.50</td>
<td>1.58</td>
</tr>
<tr>
<td>SD</td>
<td>.60</td>
<td>.57</td>
<td>.58</td>
</tr>
<tr>
<td>N</td>
<td>14</td>
<td>15</td>
<td>29</td>
</tr>
<tr>
<td>11 years:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \bar{X} )</td>
<td>1.91</td>
<td>1.63</td>
<td>1.77</td>
</tr>
<tr>
<td>SD</td>
<td>.58</td>
<td>.69</td>
<td>.63</td>
</tr>
<tr>
<td>N</td>
<td>15</td>
<td>15</td>
<td>30</td>
</tr>
</tbody>
</table>
is his older brother John who needs some money because he will be going on an important field trip soon. One day the family was trying to think of a project they could all do together. They decided it would be fun to go for a hike and pick berries. When they were all done, father worked hardest and picked 4 baskets of berries, mother picked 2 baskets of berries, their son Bob picked 2 baskets of berries, and his older brother John picked 2 baskets of berries. They brought the berries to the side of the road and sold their berries at a roadside stand and got a lot of nickels. The family had to decide how to split up the nickels. What do you think is the best way to split up the nickels?

Remember—the father worked the hardest in picking the most berries, the mother is the oldest person in the family, Bob wanted those nickels more than anyone else in the family, and John was going on an important school trip and needs the nickels most.

It should be noted that the conditions of "hard worker," "need," "oldest," and "want" were randomly assigned to characters on this and the other dilemma. The exception to this is that, for example, the same character could not be assigned as "hard worker" on both dilemmas. This would occur even though the child may have chosen the father for a reason other than his being a hard worker. Our randomization procedure eliminated this potential source of measurement error. Other cautions that exist in the peer DJS were built in here—namely, equivalent length of statements across stages read to the children, the use of a consistency check, varying the oldest character (father is oldest in the other dilemma), randomized ordering of pictures on the page, and randomized ordering of picture pairs.

One difference between the family and peer DJS was in the use of different forms. To emphasize the nature of a family, we always had one child in the pictures with the same gender and age as the subject child. Because age and gender of the other sibling may influence the subject child’s responses, we systematically varied the other sibling in four possible presentations for males and four possible for females. The eight variations are in Table 3. Each child was randomly assigned to one of the four conditions depending on his or her gender in a blocking design. Therefore, approximately equal numbers of subjects were represented across the eight conditions.

Procedure.—To control for order effects, the following conditions were counterbalanced across age and gender: (1) both peer stories followed by both family; (2) both family, then both peer; (3) peer, family, peer, family. Approximately one-third of the children in each of the eight cells of Table 3 were randomly assigned to one of the three conditions.

Approximately half the children (approximately equal numbers of males and females) were individually administered the tasks by an adult white male, and the other half by an adult white female. Both interviewers were trained in the administration of the DJS and both were blind to specific ideas being explored in the study.

Results and discussion.—Internal consistency again was adequate. The percentage of time children matched stage on the two peer DJS dilemmas was 54% (significantly different from chance at p < .05 by the binomial z statistic). The internal consistency for the family DJS was 38% (p < .05). Because differences across different DJS contexts would be compared, we first assessed synchrony within each DJS context. Synchrony within peer dilemmas was strong (.70 via the Spearman-Brown formula), and there were no mean differences between the peer dilemmas via a repeated-measures t test. The family dilemmas were also strongly related to each other (.55 via the Spearman-Brown formula) and showed no differences via the repeated-measures t test.

To test the strength of the relation between reasoning in the peer and family areas, a zero-order correlation and a partial correlation (with age) were run. Both were strong and significant (.56, p < .001, and .40, p < .001, respectively). Although these analyses in isolation might suggest synchronous development, repeated-measures analyses suggest otherwise.

1 No randomization procedure could prevent the child from choosing father both times. This is a problem with all forced-choice tests. But, on the second dilemma, father’s condition was Level 0-B (biggest); thus, the child’s choosing father both times would reduce the child’s score and reduce reliability of the instrument, and if this happened often, the low reliability would lead to an abandonment of further analyses.

2 This condition actually had four variations, as follows: (a) peer, family, peer, family; (b) family, peer, family, peer; (c) family, peer, peer, family; and (d) peer, family, family, peer.
TABLE 3  
VARIATIONS IN THE FAMILY DISTRIBUTIVE JUSTICE SCALE PICTURES  

<table>
<thead>
<tr>
<th></th>
<th>Male Subjects</th>
<th>Female Subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. One sibling = same age and gender as $S$</td>
<td>other sibling = older brother ($N$ for this condition = 9)</td>
<td>5. Other sibling = older brother ($N = 9$)</td>
</tr>
<tr>
<td>2. Other sibling = younger brother ($N = 9$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Other sibling = older sister ($N = 10$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Other sibling = younger sister ($N = 9$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. Other sibling = younger brother ($N = 10$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>7. Other sibling = older sister ($N = 10$)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>8. Other sibling = younger sister ($N = 9$)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* Because all conditions contained a pictured sibling who was the same age and gender as the subject child, it will not be referred to in the other seven conditions here.  
^ Older siblings were always larger and the younger were always smaller than the same age/same gender child pictured.

TABLE 4  
MEANS AND STANDARD DEVIATIONS FOR THE PEER AND FAMILY DISTRIBUTIVE JUSTICE SCALES  

<table>
<thead>
<tr>
<th>Grade</th>
<th>PEER DJS</th>
<th>FAMILY DJS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Male</td>
<td>Female</td>
</tr>
<tr>
<td>Kindergarten:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\bar{X}$</td>
<td>.69</td>
<td>.65</td>
</tr>
<tr>
<td>SD</td>
<td>.68</td>
<td>.68</td>
</tr>
<tr>
<td>N</td>
<td>17</td>
<td>18</td>
</tr>
<tr>
<td>Third grade:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\bar{X}$</td>
<td>1.41</td>
<td>1.48</td>
</tr>
<tr>
<td>SD</td>
<td>.51</td>
<td>.63</td>
</tr>
<tr>
<td>N</td>
<td>20</td>
<td>20</td>
</tr>
</tbody>
</table>

To test the differences between peer and family DJS, a 2 (age) × 2 (gender) × 3 (order of dilemmas) × 2 (peer DJS vs. family DJS as the repeated measure) repeated-measures ANOVA was run. There were two significant results: the main effect for age, $F(1,63) = 38.22$, $p < .001$, and the main effect for the repeated measure favoring the family DJS over the peer, $F(1,63) = 10.24$, $p < .002$. No interactions reached significance. See Table 4 for means and standard deviations. The results show that not only do both peer and family DJS scores advance with age, but also the family DJS is significantly higher at each age. That is, while there was a developmental advance from age 5 to age 9 within either context, the family context generated consistently higher distributive justice scores at each age, as Table 4 indicates. The repeated-measures analyses, then, suggest a systematic asynchrony, with reasoning about family advancing ahead of reasoning about peers. Further, one cannot claim that subjects were using conventional rather than justice reasoning in that they usually chose father or mother regardless of the stage they represented in a given dilemma. Among the 5-year-olds, for example, 12 children chose mother on the berry-picking

* Order of dilemmas refers to the following three presentation patterns: (1) both peer dilemmas followed by both family; (2) both family followed by both peer; and (3) the splitting of peer and family dilemmas. For each of the three conditions, the four dilemmas were averaged to test the main effect of condition. It was thought necessary to test this because family DJS was expected to be higher, and therefore both family dilemmas being presented first may bias the subsequent peer scores. Conversely, peer first may depress subsequent family scores. As the ANOVA shows, no such bias occurred. The level of analysis was not brought down to the eight conditions in Table 3 because of small cell sizes.
to keep the complexity of the peer and family task performance. The attempt was made here to equate demands across the peer and family stimuli. The test structure was used for both. Therefore, tasks virtually identical. In fact, the same kind of interaction of cognitive complexity and stimulus demands is present. The interaction of cognitive complexity and stimulus demands may develop asynchronously, depending on the nature of the context. When comparing reasoning in the peer and family areas, the family context pulls children to higher stages than does a peer context. This is consistent with the social-psychological findings that families operate at a higher justice level than do peer groups (Peterson, 1975).

Several conclusions are suggested from the data. First, the claim of contextual synchrony in social cognitive development is often made by examining significant correlations (see Enright & Lapsley, 1980; Urberg & Docherty, 1976). As the present study suggests, an exclusive adherence to correlational techniques is unwarranted and leads to biased conclusions. Although complexity of reasoning in the peer and family areas are related, there are clear differences between them. Without a concomitant examination of mean differences, one might be tempted to claim from correlational analyses that synchrony characterizes development when, in fact, asynchrony more accurately describes the developmental pattern. And this asynchrony is most evident when comparing reasoning about peers with reasoning about families; comparisons within peer or within family showed synchronous patterns (see repeated-measures t test data).

Second, the contexts were not confounded by a competence-performance difference across tasks (see Flavell & Wohlwill, 1969). The latter idea concerns an inverse interaction of cognitive complexity and stimulus complexity; the more complex the stimuli, the less complex the subject appears to be on the task performance. The attempt was made here to keep the complexity of the peer and family tasks virtually identical. In fact, the same kind of test structure was used for both. Therefore, a difference in competence-performance task demands across the peer and family stimuli seems unlikely. Also, both were done within the hypothetical realm rather than one referring to the child's actual friends or family. Therefore, the possible differences attributable to familiar as opposed to remote stimuli are not an issue here (see Freedman, 1974).

Third, the results cannot be explained by stimulus context alone. Not only was there a main effect favoring family over peer means, but there also was an age effect without an interaction between age and context. This shows that in the sample, 5-year-olds were more advanced in reasoning about families than about peers, 9-year-olds showed a similar pattern, and the 9-year-olds were higher than the 5-year-olds. In other words, both peer and family reasoning developed. If context were exclusively or even primarily responsible for the complexity of reasoning, we should have seen only a main effect for peer versus family (without an age effect), or at least an age \times context interaction in which the peer DJS would show development across age but the family DJS would be consistently high (because the family context, according to this theory, induces reciprocal thought regardless of age). Neither pattern occurred.

Finally, we do not have a picture of consistent imposition of one's highest cognitive structure to all problems as the structured-whole hypothesis would imply. Nor do we have a consistent imposition of certain cognitive structures in particular contexts as social-psychological theory (Lerner, 1974) seems to imply. Instead, the data suggest an interaction between the cognitive developmental and social-psychological theories in that the child develops "faster" in one context than another. And even though context seems to influence response, that response still appears to have a developmental constraint upon it.

It should be stressed that this idea is different from the notions put forward by Damon (1977) and Selman (1976), who made the claim that reasoning in different domains (e.g., logical reasoning in comparison to distributive justice) develops asynchronously. It is being claimed here, in contrast, that even within the domain of distributive justice, reasoning structures may develop asynchronously, depending on the nature of the context. When compared to the Study 1 results, it seems that the more immediate context of family may have a greater pull or influence on development than the more abstract or amorphous cultural norms that a concrete reasoner may fail to grasp.
Study 3

There were two main purposes of Study 3. As previously stated, no distributive justice investigations have been based on designs more complex than simple longitudinal ones. The longitudinal sequence allows for replications of age-stage progressions by comparing two or more longitudinal designs (see Baltes, 1968). A second objective of this study was to examine such longitudinal patterns within early childhood and compare those patterns with middle childhood ones. For example, Damon (1980) described longitudinal change for subjects between the ages of 4 and 9 without examining whether younger children grow at a similar rate as older children. We thus do not yet know whether change is gradual and continuous across childhood, or whether there are growth spurts and plateaus. This study examined change within early childhood and then within middle childhood to explore such patterns.

Subjects.—Eighty-four predominantly white and middle-class 5-year-old and 9-year-old children were assessed at 1-year intervals. Table 5 shows the breakdown by grade, gender, and cohort. Cohort 1 was tested at 5 years old in 1978 (M = 5.6, range = 5–6) and again at 6 years old in 1979 (M = 6.5, range = 6–7). Cohort 2 was tested at 5 years old in 1979 (M = 5.7, range = 5–6) and again at 6 years old in 1980 (M = 6.7, range = 6–7). The same pattern accrued for the 9- and 10-year-old cohorts 3 and 4 (M for both was approximately 9.4 at time 1 and 10.5 at time 2, ranges = 9–10 and 10–11, respectively). One child in the first cohort group and two in the second had their data removed from analyses because of inconsistency on the DJS.

Instruments.—The DJS was again used. The only difference in the scale in comparison to Studies 1 and 2 is that no 2-B items were used. Study 3 began 2 years before the others, before 2-B was devised and validated. To assure comparability of results across time and cohort, it was thought best not to include 2-B in the final year of testing when that item was available.

Procedure.—Each year (1978–80) three interviewer well trained in the administration of the DJS did the assessments. Each interviewer was blind to a child’s score and to group means of the previous year.

Results and discussion.—Internal consistency was adequate by the binomial z procedure. The percentage of time both 5-year-old cohorts matched stage on the two DJS dilemmas was 43%, p < .05. The percentage for both 9-year-old cohorts was 67%, p < .05.

To examine developmental trends, a 2 (cohort) X 2 (gender) X 2 (time 1 and time 2) repeated-measures ANOVA was run for each of the two age groups. We thus had two such ANOVAs to examine growth: 5–6 years old as a separate analysis, and 9–10 years old. The critical test was the gain from time 1 to time 2. For the 5–6-year-old group, only gain (time 1 to time 2) was significant, F(1,28) = 4.78, p < .04. Means and standard deviations are in Table 6. No gender or cohort differences were apparent. For the 9–10-year-old group, no significant main effects or interactions were observed.

On inspection of the data, it became clear that the above analyses were not the only ones or even the most appropriate ones to run. In the 5-year-old cohorts, six subjects were already at the highest stage at first testing; for the 9-year-old cohorts, 19 were at the ceiling at time 1. We reran the above repeated-measures ANOVAs without these subjects, since they could not improve their scores. It should be pointed out that by removing ceiling subjects the remaining subjects may not be characteristic of children at the developmental levels of interest. These analyses showed both longitudinal sequences with a significant time 1 to time 2 gain. No other effects were significant. The gain statistics are as follows: 5–6 years old, F(1,22) = 15.63, p < .001; 9–10 years old, F(1,29) = 5.23, p < .03. The new means and standard deviations are in Table 6.

Two different patterns emerged as a result of the two different analyses. The first

<table>
<thead>
<tr>
<th>Cohort and Grade</th>
<th>Males</th>
<th>Females</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cohort 1 (tested in 1978 and 1979) (K)</td>
<td>8</td>
<td>10</td>
<td>18</td>
</tr>
<tr>
<td>Cohort 2 (tested in 1979 and 1980) (K)</td>
<td>6</td>
<td>8</td>
<td>14</td>
</tr>
<tr>
<td>Cohort 3 (tested in 1978 and 1979) (4)</td>
<td>14</td>
<td>15</td>
<td>29</td>
</tr>
<tr>
<td>Cohort 4 (tested in 1979 and 1980) (4)</td>
<td>11</td>
<td>12</td>
<td>23</td>
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TABLE 6
MEANS AND STANDARD DEVIATIONS FOR THE DISTRIBUTIVE JUSTICE SCALE IN TWO COHORTS

<table>
<thead>
<tr>
<th>Age</th>
<th>5\textsuperscript{a}</th>
<th></th>
<th>6</th>
<th></th>
<th>9</th>
<th></th>
<th>10</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(\bar{X})</td>
<td>SD</td>
<td>(\bar{X})</td>
<td>SD</td>
<td>(\bar{X})</td>
<td>SD</td>
<td>(\bar{X})</td>
<td>SD</td>
</tr>
<tr>
<td>1978 cohort</td>
<td>.85</td>
<td>.61</td>
<td>1.28</td>
<td>.48</td>
<td>1.45</td>
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<td>.38</td>
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<td></td>
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<td></td>
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<tr>
<td>1979 cohort</td>
<td>1.16</td>
<td>.62</td>
<td>1.37</td>
<td>.47</td>
<td>1.53</td>
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</tr>
<tr>
<td>1978 cohort (2.0 removed)\textsuperscript{b}</td>
<td>.62</td>
<td>.33</td>
<td>1.27</td>
<td>.49</td>
<td>1.20</td>
<td>.30</td>
<td>1.34</td>
<td>.35</td>
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<td>((N = 15))</td>
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<td></td>
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<tr>
<td>1979 cohort (2.0 removed)\textsuperscript{b}</td>
<td>.93</td>
<td>.49</td>
<td>1.34</td>
<td>.48</td>
<td>1.17</td>
<td>.21</td>
<td>1.39</td>
<td>.51</td>
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<tr>
<td>((N = 11))</td>
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</tbody>
</table>

\textsuperscript{a} Each pair of ages represents a one year longitudinal study.

\textsuperscript{b} This represents the group in which subjects scoring 2.0 at first testing had their data removed.

analysis, run with all subjects, suggests that the youngest children, those with the lowest mean, tend to change the most after 1 year. This finding is different from Damon's (1980), whose data indicated that growth after 1 year is minimal. Recall that he collapsed all data for 4–9-year-olds. Without a more detailed analysis within a given age, as was done here, we too may have concluded the same. Instead, we found that change over 1 year was strong from age 5 to age 6.

The second pattern that excluded those at the ceiling during time 1 suggests that subjects in early and middle childhood grow in distributive justice development after 1 year. The growth, on the average, is between a half stage (early childhood) and a quarter of a stage (middle childhood) during that time. These patterns are not cohort specific but are instead characteristic of all four cohort groups. As the means in Table 6 show, the elementary school years are characterized by Level 1.0 (treating all the same) and Level 1.5 (the harder workers get more) reasoning. One further qualification needs to be added. Since only a single year separated the longitudinal sequences in this study, it is likely that the present design did not afford the most powerful test of possible cohort effects on distributive justice development. While the results of Study 3 offer considerable replication of Damon's (1980) longitudinal findings, they should be seen as offering only a preliminary analysis of cohort differences.

The individual patterns of Table 7 generally support the developmental conclusions with one important modification. Nine of the 9- and 10-year-olds changed from Level 2.0 to Level 1.0. This happened too frequently to claim that measurement error alone is the culprit; measurement error would disperse scores, not cluster them. It may be that during the important transition years to adolescence, some children once again believe that equality is more important than need. If so, we are not looking at a rigid, invariant sequence at all. Instead, we may have a general upward progression that fluctuates in the transition years to adolescence. Beyond this, the 9–10-year-old children's pattern suggests stability (26 subjects) and upward change of approximately one-half stage (11 subjects). The 5–6-year-old children, on the other hand, often showed full-stage growth (seven children) as well as half-stage (seven children). Only rarely was there greater growth than this (four children). The other downward changes besides the one above are in line with the relative infrequency of this pattern in the Kohlbergian moral domain (see Kuhn, 1976; Rest, Davison, & Robbins, 1978).

**General Discussion**

The construct of distributive justice development for the most part withstood the validation tests performed here. It appears that the development of distributive justice is a robust construct; the stage progression has been validated in the United States and Sweden as well as in Africa (see Enright et al., 1980). Not only is the rate of growth similar in the United States and Sweden, but the necessary Piagetian reciprocity structures operate similarly in both cultures. Piagetian reciprocity precedes distributive justice reciprocity in both cultures.

Distributive justice reasoning seems also to vary with the peer or family context of rea-
soning. That is, one cannot describe a child as advanced or delayed without specifying the distributive context. A child who does not display advanced levels of reasoning may actually reason more complexly given a particular issue or context. Reasoning about families brings out higher levels of distributive justice than reasoning about peers, as first hypothesized by social psychologists (i.e., Lerner, 1974). The finding is a caution to researchers in this area; one cannot stage-type a subject without qualifying the context.

Finally, distributive justice development does not seem to be an artifact of the cross-sectional design limitations of previous research. Our analysis indicates the operation of no cohort effects, though again, we hasten to caution that this analysis should be considered preliminary, since only a single year separated our longitudinal sequences. Distributive justice seems to develop in spurts—very quickly in early childhood, and more slowly in middle childhood. In addition, those in middle childhood who have not yet reached the ceiling continue to develop.

The one unexpected finding was the relatively large numbers of 10-year-olds who changed from 2.0 (need) to 1.0 (equality) reasoning. Damon (1975) suggested that distributive justice is not rigidly stagelike, and our finding supports that. But why should this particular pattern emerge? As a speculation, it could be tied to the peer-conformity demands of late middle childhood and early adolescence. Social experiments for decades have shown that peer conformity peaks at 11–13 years old and gradually declines thereafter. Many have found such a pattern (see, e.g., Berndt, 1979; Coleman, 1980; Costanzo & Shaw, 1966; Hartup, 1983). In all cases, those who conform are concerned that everyone, including the self, behave in the same way. Recall that our Study 2 here suggests the importance of context. Given peer stimuli and given that most of our older subjects were 11 years old or approaching it, the pull of peer conformity may have influenced their responses.

Level 1.0 is the peer-conformity stage in which the subject treats everyone the same. In the cohorts with 10-year-olds there were 24 of 52 children in this stage. Damon’s (1980) recent longitudinal study did not show pronounced regression to Level 1.0, possibly because his research included subjects only up to age 9, before peer conformity is strong. Fur-

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their research must examine whether those approaching adolescence who score 1.0 also evidence the most conformity to peer pressure (Costanzo & Shaw, 1966). If so, we may need to revise our thinking about general development in this domain to make allowance for this early adolescent transition.

Reference Notes


References


Kuhn, D. Short term longitudinal evidence for the sequentiality of Kohlberg’s early stages of moral judgment. Developmental Psychology, 1976, 12, 162–166.


Children’s Perceptions of Procedural Justice

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Gold, Laura J.; Darley, John M.; Hilton, James L.; and Zanna, Mark P. Children’s Perceptions of Procedural Justice. Child Development, 1984, 55, 1752–1759. This investigation concerns children’s judgments about procedural justice. First- and fifth-grade subjects were presented with a hypothetical case involving a child accused and punished by her mother for breaking a vase. Two components of the story were manipulated to vary the justness of the procedures followed in arriving at punishment. Half of the subjects heard a version of the story in which a witness to the event was present but not consulted by the mother. For the other half of the subjects no witness was present. Orthogonal to this manipulation, half of the subjects heard a version of the story containing an alternative explanation for how the vase could have been broken. For the other half of the subjects the alternative explanation was absent. The results indicate that both first and fifth graders are sensitive to manipulations of procedural justice. Particularly, when an alternative possible perpetrator is present, punishment is judged more unfair. Although age differences emerged, these differences appear to be in the comprehension of the event rather than the application of different processing rules to the information given. The relevance of these findings to developmental accounts of children’s moral reasoning and to the general issue of procedural justice is discussed.

The moral development of children has been studied frequently, but the research has been concentrated in relatively few areas of “moral reasoning.” One understudied area is that of “due process” or “procedural justice,” which subsumes a family of concepts such as the necessity of proof beyond a reasonable doubt, a weighting of the evidence by unbiased decision makers, a presentation in court of all relevant evidence, and so on.

This article reports a study that examines first- and fifth-grade children’s perceptions of procedural justice. Children were presented a hypothetical case in which a child, who circumstantial evidence suggests might have committed a “crime,” is punished by a parent. Subjects were then asked to indicate whether they believed the punishment to be fair and the child guilty. The evidence against the child was always circumstantial, and the parent’s investigation of the evidence was cursory. For some of the respondents, an element of the story suggests a plausible alternative culprit. Also, some of the respondents (this variable was manipulated orthogonally to the first) were told that a witness had been present but was not consulted about what happened.

Obviously, the alternative-culprit manipulation attempted to vary the procedural standard of proof beyond a reasonable doubt, and the ignored-witness manipulation concerned the procedural requirement for the judge to consider all relevant evidence. What can be said, from psychological theory or research evidence, about the relation between either of these procedural variations and the developmental unfolding of the concept of due process?

First, let us consider the classic theoretical perspective on authority, Piaget’s (1932) “morality of constraint” characterized by the egocentric stage. At this stage, right is defined as obedience to the will of authority and is imposed on the child by the adult. This argues that younger children should judge an act to be bad if the act’s perpetrator is punished
by an authority. Therefore, in our case, the younger children should be more certain that the child did cause the vase to break and to regard the parent's administration of punishment as more fair than the older children. Second, because they base their judgments on the judgments of the authority and do not recognize the possibility of unjust punishment, younger children should be relatively insensitive to variations in the circumstances surrounding the breaking of the vase.

In contrast, recent investigators have found that even younger children are able to make complex distinctions about what we might call the "moral circumstances" that lie behind harm-doing actions—provided the information is presented to them in an explicit and salient fashion so that it remains available in memory (Bearison & Isaacs, 1975; Chandler, Greenspan, & Barenboim, 1973; Feldman, Klooson, Parsons, Rhodes, & Ruble, 1976; Nelson, 1980; Nummedal & Bass, 1976). When these conditions are met, younger children have been able to vary their judgments of harm-doing acts in accordance with variations in the moral acceptability of the reasons for the harm-doing action (Darley, Klooson, & Zanna, 1978), the accidental versus intentional motive for a harm-doing act (Kamiol, 1978; Suls & Kalle, 1979; Suls, Gutkin, & Kalle, 1979; Zanna & Darley, Note 1), and even the cues provided by the perpetrator's happy or sad affect about whether the harm-doing act was intentional or accidental and regretted (Rybash, Roodin, & Hallion, 1979). The general patterns that emerge from these studies suggest that even younger children (kindergarten and younger) are able to use many sources of information beyond material damage in judging the morality of a harm-doing act (for a more complete review, see Darley & Zanna, 1982).

As mentioned, in the present case the parent's punishing action was offensive, from the perspective of procedural justice (Thibaut & Walker, 1975). In all versions of the story, the parent violated procedural standards by failing to give the child a chance to account for her behavior, by mingling the roles of judge, jury, and prosecutor, and by convicting on circumstantial evidence. Extrapolating from the research reviewed earlier, one might expect that younger children would be alert to considerations of procedural justice when violations are salient and explicit, but would be less likely than older children to discern subtler cues that imply procedural unfairness. Thus, while the alternative culprit manipulation was expected to influence all children, the impact should be greater on the younger ones. Conversely, the witness manipulation was expected to have greater impact on the older children since noticing that the sister can serve as a witness would seem to be a more abstract task.

The fact that the Piagetian view of the moral perspective of the egocentric child was the source of insights about the possible judgment patterns of the study (as it has been in the entire social judgment literature) guided the choice of age groups. First graders have generally been regarded as being in the egocentric age group (Piaget, 1932), whereas fifth graders are regarded as being more decen-trated. In the school systems within which the work was done, both grades were available in the same primary school, ensuring that both research samples were drawn from the same population.

Method

Subjects

Forty-four first graders (6-7 years old) and 44 fifth graders (10-11 years old) from two public elementary schools in Montgomery Township, New Jersey, served as subjects. The subjects, who participated in the fall of the school year, were predominantly white, middle-class children and included 48 females and 40 males. (No sex differences emerged, and they will not be mentioned further.) Parental permission was obtained for each child's participation in the study on the basis of a full description of the research. Additionally, the children were each given a description of the research and were asked if they were willing to participate.

Procedure Overview and Design

The procedure consisted of three sections. First, for each subject we determined what they would consider a moderate punishment. (Since different children might have different perceptions of punishment, it seemed important to attach to the story we would later tell them a punishment that was phenomenologically equal in magnitude for all children.) Second, we told all children some stories in which another child either did or did not commit a harm-doing act, but was always punished for that act. We then asked our children about the fairness of that punishment. These stories served to make the children familiar with the scales that would be used to assess the responses to the final test. Finally we told each child the slide-illustrated test story in which they heard that another child had been punished by a parent for the household "crime" of breaking a vase. Sub-
jects reported whether they believed that the child had broken the vase and whether the parent had acted fairly in punishing the child. Each subject heard one of four versions of the story. Half of the subjects were told that the family pet was also playing in the room, thus providing an alternative explanation for how the vase could have been broken. Orthogonal to this, half the subjects were told that a potential witness was available—an older sister studying nearby. (If the older sister was present, she was not questioned by the parent.) First and fifth graders were randomly assigned to the four conditions in this (witness present vs. witness absent) x 2 (cat present vs. cat absent) design.

**The Punishment Scaling Procedure**

Each subject was tested individually by a female experimenter in a room provided by the school. Upon entering, the subject was greeted by the experimenter and asked to sit in a chair facing the experimenter. The punishment scale procedures were given in the first part of the test session. After giving name, age, and number of older and younger siblings (no birth order effects emerged, and they will not be mentioned further), the subject was asked to rank three punishments in terms of severity. Specifically, subjects were presented pairs of the following alternatives: getting spanked, getting sent to one's room, and getting yelled at. They were asked to report which of the pair was the most severe. If the subject's three paired choices created a transitive ordering of the stimuli, the middle-level punishment was selected to be used as the punishment the parent assigned in the test story. For the three cases in which the subjects' rankings were inconsistent, the experimenter randomly chose one of the three punishments to insert in the story.

**Premeasures of the Child's Judgments**

To acquaint the subjects with the measures to be used, the children were next told two episodes in which a child did or did not disobey a parent, and two episodes in which this was ambiguous. After each episode the subject answered four questions about the incident: (1) Do you think the child committed the forbidden act? (2) Do you think the child did anything wrong? (3) Do you think it would be fair for the child's father to punish him? (4) Why would it be fair/unfair?

The first questions were to be answered with responses of "yes," "no," or "I don't know." If the subjects answered yes or no, they were asked to specify whether they were "very sure" or only "pretty sure." Therefore responses to each question could be scaled along a five-point continuum. The fourth question was open-ended, and the experimenter transcribed the response.

**The Test Story**

After responding to the premeasures, each child was read one of four variations of the test story accompanied by slides depicting story actions. Subjects in the witness-absent/cat-absent condition heard the following version (which was the core story common to all conditions):

"A little girl named Susan was playing with her dolls in the living room of her house. (1) Susan was playing near a table. On the table was a bottle with flowers in it. (2) Susan's mother went out to water the flowers in the backyard. (A) The mother finished watering the flowers and she came back into the house. She looked into the living room and saw Susan there. (3) For the first time, the mother noticed that the bottle was broken and lying in pieces. (4) The mother did not ask anyone anything. First she said to Susan, 'You were not being careful, and you broke my bottle.' Then she punished Susan by (yelling at her/sending her to her room/spanking her)."

At the end of the story the subject was asked eight questions about it. The first four were identical in form to the four asked about the earlier stories. (The context was altered and reflected the particulars of the present story—e.g., "Do you think Susan broke the flower bottle?") Four additional questions were asked as checks on the subject's percep-

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1 The administration of the premeasures, as well as serving to give the subjects practice with the main task, creates possibilities of multiple analyses relating aspects of the premeasures to inferences on the final task. We carried out a number of such analyses and found no findings of sufficient interest to warrant taking journal space to report. The interested reader is invited to write to the corresponding author for further details.
tions of the independent variables: Could the bottle have been broken some other way besides Susan breaking it? (scored yes, don’t know, and no); How else could the bottle have been broken? (open-ended question); Could anyone besides Susan have seen how the bottle got broken? (scored yes, don’t know, and no); Who else could have seen how the bottle was broken? (open-ended question); After answering these questions the session was complete. The subjects were thanked for their participation and escorted back to the classroom.

The alternative-explanation manipulation.—Half the subjects were told about the presence of the family pet at the scene of the “crime.” This provided a specific possible alternative explanation of how the bottle might have been broken. To accomplish this, at point A in the story above, the following extra material was inserted: “Tiger, the family cat, came into the living room. He played near Susan and her dolls by the table.” (Here a slide was shown that pictured the cat actually sitting at one end of the coffee table.)

The witness manipulation.—Orthogonal to the alternative-explanation manipulation, half the subjects were told that another person was present in the room, who had watched the child play and was therefore a potential witness to the event. The following sentences were inserted in the story, also at point A: “Susan’s older sister, Mary, was doing her homework near the living room where Susan was playing. [Here a slide was shown, taken from behind the older sister, showing her back, her homework on the table in front of her, and in the background, the coffee table at which Susan played.] Mary looked up and sat in her chair and watched Susan play.”

For conditions in which either the cat or the older sister were presented, a phrase was inserted in the story that made clear that the mother was aware of it.

Results

Checks on the Manipulation

Effects of the witness.—Subjects responded to the question, “Could anyone besides Susan have seen how the bottle got broken?” on a three-point scale (1 equals “no,” 2 equals “don’t know,” and 3 equals “yes”). A main effect for the witness manipulation was found, indicating that subjects in the sister-present conditions were more likely to report that someone else could have seen how the bottle was broken (present = 2.89, absent = 1.50), \( F(1,80) = 91.65, p < .001 \). The grades level of the subjects, the presence or absence of the cat, and the interactions were not significant for this measure. As expected, analysis of the open-ended question asking who else could have seen how the bottle was broken showed that 40 of the 44 subjects in the witness-present conditions cited “Mary” as a witness, while 33 of the 44 subjects in the witness-absent conditions stated that “no one else” could have seen the act.

Effect of the cat.—On the question, “Could the bottle have been broken some other way besides Susan breaking it?” subjects in the cat-present condition were more likely to state that there was some other way the bottle could have been broken than were subjects in the cat absent conditions, \( F(1,80) = 24.59, p < .001 \). Subjects in the cat-present conditions always responded that there was some other way that the bottle could have been broken (\( M = 3.0 \) on a three-point scale), and even subjects in the cat-absent conditions responded above the midpoint of the scale (\( M = 2.34 \)). A second main effect was found indicating that fifth graders were more likely to conclude that there was some other way that the bottle could have been broken (M = 2.82) than were first graders (M = 2.52), \( F(1,80) = 4.94, p < .05 \). However, this main effect is best interpreted in light of a significant interaction between the subjects’ grade levels and the cat manipulation, \( F(1,80) = 4.94, p < .05 \). While both first and fifth graders concluded that there was some other way that the bottle could have been broken when the cat was present, first graders were less certain that there was an alternative when the cat was not present (M = 2.05 vs. 2.64), \( t = 2.18, p < .05 \). Open-ended responses to this question are discussed later.

Judgments of Guilt and Fairness

The responses to three of the questions (“Do you think Susan broke the bottle?” “Do you think Susan did anything wrong?” and “Do you think it was fair of Susan’s mother to punish Susan?”) were found to be more highly correlated for first graders (“break” with “wrong,” \( r = .88, p < .001 \); “break” with “fair,” \( r = .95, p < .001 \); and “wrong” with “fair,” \( r = .86, p < .001 \)) than for fifth graders (“break” with “wrong,” \( r = .67, p < .001 \); “break” with “fair,” \( r = .67, p < .001 \); and “wrong” with “fair,” \( r = .62, p < .001 \)) for both first and fifth graders. For first graders (\( a = .96 \)) and fifth graders (\( a = .85 \))
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