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Strong Evidence for Universals in Facial Expressions: A Reply to Russell's Mistaken Critique

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J. A. Russell (1994) misrepresents what universality means, misinterprets the evidence from past studies, and fails to consider or report findings that disagree with his position. New data are introduced that decisively answer the central question that Russell raises about the use of a forced-choice format in many of the past studies. This article also shows that his many other qualms about other aspects of the design of the studies of literate cultures have no merit. Russell's critique of the preliterate cultures is inaccurate; he does not fully disclose what those who studied preliterate subjects did or what they concluded that they had found. Taking account of all of Russell's qualms, my analysis shows that the evidence from both literate and preliterate cultures is overwhelming in support of universals in facial expressions.

It is rare when anyone takes a large body of research seriously enough to write as extensively as Russell (1994) has done, and for that I am grateful. Regrettably, he omits crucial information in his report of published research. I discuss 17 such instances; for example, not telling the reader what an investigator concluded his or her study had shown. Russell also misunderstands or misrepresents many of the findings that he does report. I discuss 8 such instances; for example, criticizing an investigator for failing to perform a statistical test that the investigator had actually performed. These are not all of Russell's errors, but space limitations imposed by the editors of this journal required that I select only the more egregious ones. There were also a few honest disagreements that I explicate, but they are not the main problem with Russell's article.

Because Russell's article is long and raised many questions about many issues, there is a risk that few will read it in its entirety. The danger then is that simply because of its bulk, and because it is published in this prestigious journal, readers who consider only the opening and closing sections may mistakenly conclude that Russell's criticisms must be well grounded and the doubts he has raised about research on facial expression of emotion must have some merit. I endeavor to show that is not so.

Editor's Note. Ekman's rebuttal and the one by Izard that follows it were prepared in response to an invitation from the *Bulletin* to write replies that would appear in the same issue with the target article by James Russell. The Russell article inadvertently appeared alone in the January issue.

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Russell began with what he called a historical "sketch," in which he claimed that I, and Izard also, gave undue credit to Darwin and that I failed to acknowledge many who had taken similar positions to my own. He then delineated different meanings of the concept of universality. Next, he criticized on various grounds the findings on universality from the many studies of literate cultures, emphasizing disagreement rather than consensus. In a similar vein, he analyzed the findings from preliterate cultures and ended by suggesting various alternative interpretations of what had been found.

I have relegated to an Appendix my point-by-point refutation of Russell's misrepresentation of the past literature and his derogation of my scholarship and contribution. Instead I chose to focus primarily on the findings. Regardless of who deserves credit for what contribution, it is most important not to let stand Russell's misrepresentation and misinterpretation of what has been found. Although the majority of my reply deals with these findings, it is necessary to clarify first the many methodological and conceptual confusions in Russell's article.

My reply is organized into seven sections. (a) What question is being asked? Russell confused what are actually four quite different questions about the face and emotion and also the different methods that are used to address them. (b) What is meant by universality? Russell erected a straw man who admits no cultural differences in emotional expression. The reader would not know from Russell's account that my neuro-cultural theory of facial expression of emotions emphasizes cultural differences as well as some universals. (c) How are words related to expressions? Russell failed to recognize that cultural influences on how emotion is represented in single emotion terms should cause variations in how subjects will use words to interpret facial expressions, both within and across cultures. Despite such variations, there could be—and in fact is—important evidence of agreement as well.

The next four sections deal with the research findings. (d) What level of agreement should be required to establish universality? Only the straw man universalist, who maintains that each facial expression signals just one emotion word, would require perfect agreement among all who judged what emotion is shown in an expression. Instead, the standard should be whether the amount of agreement was statistically significant. That standard was met in more than 30 studies by many different investigators using different methods. (e)

How much agreement was obtained in the literate culture studies? A reanalysis of the data in the literate culture studies shows that agreement was extraordinarily high, much higher than what was required to reach statistical significance. (f) Are there limitations in the studies of literate cultures that discredit the findings of cross-cultural agreement? New data, and previously published findings that Russell omits, answer any qualms Russell may have created about the literate culture studies. (g) Does the preliterate culture data support universality? Russell's criticisms of the preliterate culture data are, as he acknowledges, based on what he imagines might have occurred. When the reader learns what did occur and what Russell omitted, it will be clear that this evidence survives quite unscathed.

I do not discuss the recent findings on the universality of contempt. As we wrote (Ekman & Friesen, 1986), contempt is unusual in many respects, and further, contempt was not studied as a separate emotion from disgust in many of the early studies on which the claim of universality rests.[1] Also, there have been many recent articles devoted to contempt (Ekman & Friesen, 1986, 1988; Ekman & Heider, 1988; Ekman, O'Sullivan, & Matsumoto, 1991a, 1991b; Izard & Haynes, 1988; Matsumoto, 1992; Ricci Bitti, Brighetti, Garotti, & Boggi-Cavallo, 1989; Russell, 1991a, 1991b).

What Question Is Being Asked?

Russell's review of the literature failed to distinguish among four very different questions addressed by the various investigators he discussed. (a) Do subjects agree in their judgment of facial expressions; that is, is agreement better than chance? (b) What judgment procedure will maximize the extent of agreement among subjects in their judgments of facial expressions? (c) Do facial expressions provide accurate information about emotion? (d) Is there a universal association between certain facial expressions and specific emotions?

Part of my disagreement with Russell's historical sketch is due to his failure to be clear about which investigators focused on which questions (see Appendix). This matters not only in terms of who deserves credit for which work but also because the findings on one question do not necessarily relate to findings on another. Consider these examples. Subjects might show greater agreement when they use emotion scales (e.g., *pleasant–unpleasant* or *active–passive*) to judge expressions than when they use emotion categories (e.g., *fear, anger,* or *sad*). Such a finding would be relevant to Question b, about how to maximize agreement. Scales, however, might not produce as much differentiation among emotions and, therefore, might not be most useful in studying the accuracy question or the universals question. (Note that this example is hypothetical.) Or subjects might agree within each culture about which expression is associated with which emotion (Question a) but not agree across cultures (Question d). Also, subjects could well agree (Question a) but be completely inaccurate (Question c).

Most of the research before the mid-1960s focused on the first three questions, not on the universality question. Ekman, Friesen, and Ellsworth's (1972) analysis of that literature, and of the judgments made by those who previously reviewed the literature, was that (a) agreement in the judgment of emotion from facial expression was low to moderate (Question a); (b) investigators who wanted to maximize agreement turned to scales rather than category judgments, in particular Schlosberg's (1954) three-dimensional model (which is similar to Russell's circumplex model; Question b); (c) the evidence for accuracy was inconclusive (Question c) and—apart from the anthropologists (e.g., Birdwhistell, 1970; LaBarre, 1947), who argued a cultural relativist position from examples—there were only a few experiments on universality, the findings from those experiments were inconclusive, and the design of some of those experiments was flawed (see Ekman et al., 1972, pp. 155–158). Because the issues raised by Russell pertain primarily to universality, here I consider only the evidence pertaining to that issue, which means focusing on the quantitative research since 1965.

What Is Meant by Universality in Facial Expression: Disposing of Russell's Straw Man

Russell's Position

Russell listed four propositions as relevant to what he called the "universality thesis." Russell accepts the first of these: "specific patterns of facial muscle movement occur in all human beings" (Russell, 1994, p. 106). By accepting this definition, does Russell simply mean that all humans have the same musculature and can therefore produce the same range of expressions? That does not grant anything beyond our anatomy, which was the position of the extreme cultural relativist Birdwhistell (1970).

Russell's second and third statements of the universality position I do endorse, but they do not give the entire and necessary picture of what that position is. In Russell's own words, "(b) . . . certain facial patterns are manifestations of the same emotions in all human beings, (c) . . . observers everywhere attribute the same emotional meaning to those facial patterns" (Russell, 1994, p. 106). By limiting the universality position just to these two points, the first related to how emotions are shown in the face and the second related to how the emotional meaning of an expression is judged, and by failing to include how and why facial expressions also vary with culture, Russell created a false basis for what to expect in cross-cultural studies of the judgment of facial expressions. A reader might expect perfect agreement within each culture and perfect correspondence across cultures, with no exceptions or deviations. In fact, that is the standard by which Russell evaluated the cross-cultural research he reviewed. He considered any deviation from this standard as evidence against universality. Perhaps such a standard would be appropriate if we were considering spinal cord reflexes, but it is inappropriate for emotional expressions.

Russell created this straw man so that he can set impossible standards for the absolute level of agreement that must be obtained, which enables him to discredit all the findings, because no study found perfect agreement. Only someone who admits of no cultural variations in facial expression would be challenged by findings of *any* differences in expression. It is only a straw man universalist who would be dismayed by any variation in the extent of agreement across cultures in how subjects use words to interpret the emotions shown in facial expressions. Russell cited articles (Ekman, 1972, 1973, 1989) in which my quite different view of universality is made clear, but my view is not represented in his critique.

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[1] All of my cross-cultural research on facial expression was jointly authored with Wallace V. Friesen, although some of the theoretical writings were mine alone. I use *we* when referring to joint work with Friesen, and *I* when referring to work that is just my own.

The Neuro-Cultural Position on Universality

In our earliest writing in which we considered universality, we (Ekman & Friesen, 1969) distinguished emotional expressions, in which we expected some universals and some cultural differences, from two other types of facial behavior, which we predicted would be entirely culture specific: the use of the face to emphasize or otherwise illustrate speech and the use of symbolic gestures or what we called *emblems*. Not all facial behavior is relevant to emotion, and not all facial behavior that is relevant to emotion was said to be universal.

In a few asides, Russell acknowledged our treatment of cultural differences in facial expressions of emotion, but in the main, he characterized us as having taken an extreme position. A reader of his article might not know that our view of emotional expression was, from the outset, not absolutist. We took neither a totally universalist viewpoint (such as Eibl-Eibesfeldt, 1970) nor a totally relativist viewpoint (such as Birdwhistell, 1970, or LaBarre, 1947). To make clear that what I have written here in refutation was published decades ago, I use quotes from these earlier publications.

In the first article we wrote on this topic, we said: "We agree with Tomkins and with Darwin that there are distinctive movements of the facial muscles for each of a number of primary affect states, and these are universal to mankind" (Ekman & Friesen, 1969, p. 71). Note that we did not say that *all* facial movements are universal nor did we say anything about whether observers will agree in their interpretations of those facial expressions, let alone whether such agreement will be perfect. In that same first article, we went on to say, "While the facial muscles which move when a particular affect is aroused are the same across cultures, the evoking stimuli, the linked affects, the display rules and the behavioral consequences all can vary from one culture to another" (Ekman & Friesen, 1969, p. 73). We spent many pages in that article and in subsequent publications (see, especially, Ekman, 1972, 1973) explaining how these sources of cultural variation could interfere with, and obscure, the appearance of universal expressions. After doing so, we summarized, "Our argument has been to emphasize the difficulty in uncovering the pan-cultural elements, and to caution against the danger that they may be obscured by a failure to isolate each of the variables listed" (Ekman & Friesen, 1969, p. 76). Three years later, I gave a name to our approach to facial expression: "We have called our theory *neuro-cultural* because it emphasizes two very different sets of determinants of facial expressions, one which is responsible for universals and the other for cultural differences" (Ekman, 1972, p. 212).

Given all the sources of cultural variation, it is a formidable problem to obtain *any* evidence for universality, much less perfect agreement. Next, I explain why it is especially difficult to obtain evidence of universality if the research method compares how people in different cultures use emotion labels to describe facial expressions, the paradigm used in most of the work that Russell reviewed.

How Are Words Related to Expressions?

Of all the different aspects of an emotion—expressions, physiological changes, appraisals, memories, expectations, subjective feelings, coping—the words that are used to represent that experience should be most accessible to and most subject to influence by cultural differences (Ekman, 1993; for similar arguments, also see Heider, 1991b; Izard, 1982). Does Russell believe that single emotion terms such as *fear, anger,* or *disgust* (or the valence and arousal dimensions he is more fond of) are precisely what facial expressions are supposed to convey about emotion? Or does Russell think that is my position? It certainly is not.

Here is what I have said about what is conveyed by a facial expression of emotion:

> There is no evidence about precisely what type of information is conveyed when, during an on-going social interaction, one person sees a facial expression of emotion on another person's face. . . . The fact that in an experiment people agree in selecting an emotion term for a face does not mean that people engaged in social interaction usually respond to faces in those terms. . . . People may also respond to seeing an expression by noting the antecedent event rather than utilizing an emotion term. . . . "Your heart must be pounding," would be an example of noting a known physiological accompaniment of anger expressions. "You must be remembering something terrible," would be a comment on a cognitive process associated with an emotion. Emotion terms can be thought of as a kind of shorthand, an abbreviated way to refer to a package of events and processes that comprise the phenomenon. Each emotion term, I believe, refers to a different set of organized, integrated processes. They include the antecedent events, the physiological and motor responses, the memories, thoughts, images, and information processing, and the mobilization of efforts to cope with the source of emotion. All or any of these may be implied when someone says "he looks angry." (Ekman, 1989, p. 159)

Words are superb for describing actions, directions, locations, thoughts, and so on, but emotions are hard to capture with words, particularly with single emotion terms. It is only a poet who has an easy time describing our emotions in words, and poets do not usually accomplish that with a single emotion label. Facial expressions are useless for conveying most of the information that is so easily communicated by words, but facial expressions can readily reveal emotional subtleties that are difficult to describe in words. When words are used to convey emotions, it is no wonder that it is often by recourse to analogy, metaphor, or description of preceding or consequent events. As Bertrand Russell (1961) said, "a dog cannot relate his autobiography; however eloquently he may bark, he cannot tell you that his parents were honest though poor" (p. 133). The matching up of words and facial expressions is imperfect, at least in part, because they each convey what the other cannot.[2]

We never claimed that facial expressions evolved to represent specific verbal labels. Nor did we say that the meaning of an expression is limited to or best captured by a specific, single word. We used emotion labels for a very specific purpose: to demonstrate that despite all of the problems associated with labeling the emotion shown in a facial expression, subjects would do better than chance in this task. It would have been just as relevant to have asked subjects to judge which antecedent event was associated with each face, a method we did indeed use. Later, I argue that Russell's criticism that by doing so we "confounded" (Russell, 1994, p. 127) emotions with antecedent events is based on his narrow focus, which equates emotions only with single emotion terms.

There is no reason to expect that every culture will label the emotions in exactly the same way. Instead there may be major differences among languages in how many words there are for each

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[2] I thank Nancy Etcoff for reminding me of this point and for the Bertrand Russell quote.

emotion—whether the words combine references to more than one emotion and whether the emotion terms include references not only to emotions but also to antecedents, consequences, sensations, use metaphors, and so on (see Heider, 1991b). Furthermore, even cultures that share the same language (e.g., the United States and England) may have different attitudes about emotion, which may cause the same emotion word to have very different connotations. They may even use very different metaphors and colloquialisms when referring to an emotion. These problems, which work against finding agreement within cultures and across cultures that share a common language, should be further magnified when studying cultures that have different languages and when using translations of emotion labels when asking subjects to judge emotions. Because language problems work *against* finding agreement, such findings must be considered even more strongly when they are obtained. (See Izard, 1994, p. 296, for a similar point.)

What Level of Agreement Should Be Required to Establish Universality?

No one should expect anything like perfect agreement within, let alone across, cultures in the use of single emotion terms, or even groups of what are supposed to be synonyms, for a phenomenon as elusive as emotion and as transitory and difficult to describe as an expression. The question is whether agreement across different cultural and language groups survives at all. Is it better than would be expected by chance?

Russell (1994) began his critique of the cross-cultural evidence by constructing an analysis of variance on the basis of the percentage of agreements for each emotion reported in nine studies, in which emotions were one factor and culture (Western [20 cultures] vs. non-Western [11 cultures]) was the other factor. He reported statistically significant effects for emotion, for culture, and for the Emotion × Culture interaction. Russell failed to provide important information about how much of the variance was accounted for by each effect. By our computations, it was 26% for emotion, 14% for culture, and 3% for the interaction between culture and emotion.[3]

These findings surely *do not* challenge the universality position. The neuro-cultural theory of expression never claimed that a group of non-Western cultures must attain the same level of agreement in using single emotion terms to describe facial expressions as a group of Western cultures (i.e., the culture main effect in Russell's, 1994, analysis). There are many reasons why the non-Western cultures would not reach as high a level of agreement (e.g., difficulties in finding an adequate translation for each emotion term). The important question is whether the non-Western groups by and large did reach a significant level of agreement, and the answer to that, Russell admits, is yes.

Furthermore, the neuro-cultural theory does not maintain that the degree of agreement will be the same for all emotions (i.e., the emotion main effect in Russell's analysis). For example, it is typical to find higher agreement in the judgments of happy facial expressions than about the negative emotions, in every language group. There are many reasons why this might be so. Darwin pointed out that the happiness facial expressions differ in appearance from the negative emotion facial expressions more than the negative expressions differ from each other. There might also be less ambiguity in most languages about the single emotion labels used to represent happy feelings than there is among the single emotion labels used to represent each of the negative emotions. Even if agreement for the negative emotions was lower than for positive ones, the crucial issue in terms of my position on universality is whether agreement in the labeling of the negative emotions is better than chance, and again the answer shown in Russell's Table 2 is yes. Finally, the neuro-cultural theory of facial expression never maintained that there would be no difference between Western and non-Western cultures in which emotions showed the greatest agreement (i.e., the Emotion × Culture interaction in Russell's analysis). Non-Western and Western cultures might differ in the amount of attention and lexical representation of one emotion as compared with another. Again, the critical question for the neuro-cultural theory is whether agreement was better than chance. Russell did acknowledge that better than chance agreement was obtained in the studies he summarized in his Table 2: "I do not dispute the formal statistical finding in each study of an association between facial expression and emotion label" (Russell, 1994, p. 109). I thought formal statistical findings are what settle such matters. Is agreement perfect in these studies? Of course it is not. Are there also substantial differences in the absolute level of agreement reached? Of course there are, but that does not dispute the universality finding.

The reader should remember that statistically significant findings were in no way foreordained in these literate culture studies. There could have been consistent differences in the modal emotion attributed to an expression across cultures. That is not what happened. The studies listed in Russell's Table 2 instead show just the opposite. In nearly every instance, the modal emotion attributed by one cultural group was the modal emotion attributed by another cultural group. Out of a total of 186 entries in Russell's table (6 emotions × 31 cultures) there were only 7 instances in which the level of agreement was less than 50%; only these 7 out of 186 entries in which the mode could have disagreed with the emotion judged by the majority in the other cultures. One might claim that the extent of agreement in these studies is somehow inflated by the happiness expressions (for which there was not a single example of disagreement). However, even after removing the happiness expressions from consideration, the evidence for cross-cultural agreement is still overwhelming: The majority of the subjects in every culture agreed about the emotion shown in the facial expressions in 148 out of 155 remaining entries.

Note that Russell did not present the kind of judgment data necessary to disconfirm the universality position. To do this, Russell would have to find a culture in which the subjects judged the facial expressions we have argued indicate anger, disgust, sadness, and happiness to all indicate entirely different emotions.[4] For example, the majority of subjects in this new culture would have to judge all

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[3] There are reasons to question whether Russell's ANOVA was computed appropriately. He did not take account in his computations of the different sample sizes. (I thank Erika Rosenberg for noting this problem.) When we recomputed the analyses, taking account of these sample sizes, the interaction between culture and emotion was no longer significant. Although Russell said he took account of violations of sphericity, when we recomputed his ANOVA, we found that he had not done so. (I thank Brian Knutson for noting this problem.) When we recomputed the analyses, taking account of violations of sphericity, the interaction between culture and emotion again was no longer significant.

[4] I exclude fear and surprise, because the distinction between those two emotions has not always been preserved, although these two emotions are nearly always distinguished from all of the other emotions.

of the sad expressions to be anger, all of the anger expressions to be happy, all of the happy expressions to be disgust, and so on. There is no such evidence.

To summarize, in this section, I have shown that the findings from the literate cultures provide strong support for the neuro-cultural theory of emotional expression. The findings were statistically significant among Western and among non-Western cultures. Universals in facial expression are robust, replicated phenomena.

How Much Agreement Was Obtained in the Literate Culture Studies?

Russell's (1994) analysis and discussion of findings from literate cultures, and my rebuttal in the last section, may have obscured a crucial fact. Agreement across cultures in the judgments of facial expressions was far greater than what was required to reach statistical significance.

To demonstrate that, I reanalyzed the data presented in Russell's Table 2, in which Russell lists the percentage agreement for 31 groups (in the rows) for each of 6 emotions (in the columns). My Table 1 shows the results for the 20 Western groups and Table 2 for the 11 non-Western groups.

The first row in each table shows the percentage agreement for each of the emotion categories that would be expected to occur by chance. Estimating what level of agreement would be expected by chance is not an obvious matter. The subjects in these studies were given six choices, and on that basis one could estimate chance as 1/6 or 16.6%. I have chosen a much more conservative strategy, in which the chance estimates are set much higher than that. For happy, I set chance at 50%, reflecting a 50–50 choice between a positive and negative emotion expression. For surprise, I set chance at 33.33% or 1/3, reflecting the probability of choosing "surprise" from among the emotions it is most often identified as: surprise, fear, or happiness. I used 25% or 1/4 for each of the negative emotions, because this indicates the probability of choosing any one of the four negative emotion terms for any one of the negative emotion expressions.

The second row in each table lists the minimum percentage agreement that would be required for an observed percentage agreement to be significantly greater than chance at the .001 level in a one-sample z test. The sample size used for this computation was the total sample size across all 20 Western groups in Table 1, and the total sample size for all 11 groups in Table 2. Note that the minimum percentage values in Tables 1 and 2 in this row differ because of the difference in the total sample size for the Western and the non-Western cultures.

The third row in each table lists the percentage agreements actually obtained for each emotion. These percentages were derived by using the following method. For each group listed in Russell's table, I multiplied the sample size by the percentage agreement listed for each emotion to obtain a frequency measure for the number of subjects who chose the predicted emotion in each study.[5] Then, for each emotion, I summed across all studies to obtain an overall frequency. Next, I divided this by the total sample size across studies to obtain the overall proportion of subjects across all studies (Western and non-Western studies were done separately), who chose the predicted emotion category. These proportions are listed as percentages, to be consistent with the method of presentation in Russell's Table 2.

Comparison of row 2 (the level of agreement required for statistical significance) with row 3 (the actual level of agreement obtained) shows that agreement was much, much higher than was required just to reach significance at the $p < .001$ level of confidence. In the Western groups, the agreement obtained was 40 to 50 percentage points higher than what was required for statistical significance. In the non-Western cultures, the agreement was 28 to 43 points higher than what was required for statistical significance.

To summarize the discussion of this section, agreement was extraordinarily high, far greater than what was required to establish statistical significance.

Table 1
Amount of Agreement Obtained in Judgments by Western Subjects

Measure	Happy	Surprise	Sadness	Fear	Disgust	Anger
Chance agreement	50.00	33.33	25.00	25.00	25.00	25.00
Min. % agreement for .001 sig. (1-tailed)	54.10	37.30	28.72	28.72	28.72	28.72
Observed % agreement	94.74	87.92	78.62	77.47	79.91	78.04
Diff. betw. observed agreement & min. % for .001 sig.	40.64	50.62	49.90	48.75	51.19	49.32

Note. n = 1,417. Min. = minimum; sig. = significance; Diff. betw. = difference between.

Table 2
Amount of Agreement Obtained in Judgments by Non-Western Subjects

Measure	Happy	Surprise	Sadness	Fear	Disgust	Anger
Chance agreement	50.00	33.33	25.00	25.00	25.00	25.00
Min. % agreement for .001 sig. (1-tailed)	56.35	39.60	30.90	30.90	30.90	30.90
Observed % agreement	87.80	76.63	74.27	62.42	67.41	59.10
Diff. betw. observed agreement & min. % for .001 sig.	31.45	37.03	43.37	31.52	36.51	28.20

Note. n = 586. Min. = minimum; sig. = significance; Diff. betw. = difference between.

[5] This method of obtaining overall percentage agreements takes into account the different sample sizes for the various studies. Thus, in the calculation of the overall proportions for each emotion, studies with different sample sizes are appropriately weighted. In Russell's two-factor ANOVA on emotion by culture, in which he "treated each sample as a single case" (p. 109), he did not appropriately weight studies according to sample size. Thus, the percentage agreement for a sample size of 30 was weighted equally to that of a study with a sample size of 168.

I next turn to how Russell dealt with this very strong evidence of cross-cultural agreement in the literate culture studies, devoting the bulk of his article to raising every conceivable flaw in these studies. Although no one of the purported "flaws" he raised is very convincing, I fear that the reader might be numbed by the number of pages devoted to this enterprise and conclude that there must be some serious problem with this research. I show that few of Russell's points have any merit whatsoever. Even a whole school of red herrings is still composed of red herrings.

Are There Limitations in the Studies of Literate Cultures That Discredit the Findings of Cross-Cultural Agreement?

By my count, Russell came up with 10 possible problems, which he organized under four rubrics: subjects, presentation order, facial expressions, and response format. I start with the last—response format—because of all the problems he raised; this was presented in a way that might seem convincing to readers. Another reason to focus on response format is that Russell introduced new data that he considered relevant to it, and I do so also. At the end of this section, I briefly consider Russell's other criticisms.

Response Format

Forced choice. Russell argues that the statistically significant agreement across literate cultures, summarized in his Table 2, should be questioned because most of it was gathered with a forced-choice response format. Russell raised the unlikely possibility that the truly appropriate emotion label might never have been included in the list of forced-choice options presented to subjects. To seriously shake those findings, Russell needed to assert that somehow that unlikely possibility had occurred, not only for one emotion, but for all of the emotions shown in all of those expressions. If that were so, by Russell's reasoning, subjects would have had to choose some other emotion, even though the emotion they chose would not be the one they would have chosen if only the investigator had provided them with the choice they really wanted to make. I quote from Russell:

> Consider the situation in which the list of response options fails to include a label for the observer's spontaneous categorization. . . . Given other options, subjects would have chosen other category labels. . . . In general, any short list, including the ones used in the studies of Table 2, should be suspected of being overly restrictive. (Russell, 1994, pp. 116–117)

If that really did occur and the "correct" choices were not provided, is it not remarkable that all of these subjects across all of these studies, in all of these cultures, still agreed about what term was the best among those they were offered? This is clear evidence of universality, strong and consistent agreement across cultures. However, if Russell is right and the *best* words for each expression were not included in the lists given to the subjects, we would have to add a qualification to our interpretation of the findings. We would still conclude that there was significant cross-cultural agreement about the emotions shown by facial expressions, but we would have to acknowledge uncertainty about whether the specific words for the emotions used in these studies were the very best ones. As I have explained, the specifics of which exact word is chosen is not the issue. Agreement across cultures is! However, I show that Russell's claim that the best or correct choices might not have been included is still another red herring. It can be refuted by Russell's own data as well as by other data that I introduce below.

Russell's Table 7 reports findings from a study in which he tried to find experimental support for his contention that the forced-choice response format could be misleading. I use anger as an example, but what I will say about anger holds for most of the other results that Russell reported in Table 7. Russell used a photograph of a facial expression universally judged to be anger and showed that when the word *anger* was not included among the label choices he provided to his subjects, the subjects did not choose it. In one condition, the choices he gave his subjects were joy, relaxation, surprise, fear, interest, and disgust, and the subjects chose disgust.[6] Does this finding mean that the facial expression in the photograph is not actually associated with anger, as Russell suggested?

To answer this question, consider the following example from another field of research. Suppose we have a patch of yellow, which everyone agrees is *yellow* when they are offered the label choices of *yellow, orange, blue, green, purple* and *black.* In a Russell-like study of colors, we then show subjects the same patch, but we remove *yellow* from the set of available labels, leaving the subjects a choice among the labels *orange, blue, green, purple* and *black.* Lo and behold, now most of the subjects call that yellow patch *orange.* Does that prove that the patch is really orange and not yellow? It would only show that subjects will chose a second-best label when deprived of the best label.[7] (See Izard, 1982, pp. 3–18, for a similar argument.)

For Russell's subjects who had to judge an anger expression but were given a set of labels that did not include *anger, disgust* was the most likely choice. Russell could have predicted that from previously published findings. Thirty years ago in a pioneering study, Tomkins and McCarter (1964; which Russell cited in other regards) analyzed what they called "common confusions," the emotions attributed to an expression by those subjects who did not make the judgment made by the majority of the subjects. Disgust was the common confusion for anger, the emotion attributed by the minority of subjects who did not give the anger response.[8]

Twenty-two years ago, I (Ekman, 1972) did the same type of analysis, examining the emotion label given by those subjects who did not give the majority response for each expression. My study included the judgments of subjects from five literate cultures. When the majority of the observers judged an expression to show anger, the minority of observers in *every* culture judged it to be disgust. For surprise, fear, disgust, and sadness, there was also statistically significant cross-cultural agreement about what emotion was seen by those who did not give the majority response. These findings predicted what subjects would do when Russell showed them expres-

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[6] Mark Frank pointed out that Russell's own circumplex model (Russell & Fehr, 1987) would not have predicted this specific finding. That model would have predicted that fear would occur as often as disgust in response to an anger picture, but the predominant response was disgust.

[7] I thank Nancy Alvarado (personal communication, August 1993) and Mark Frank (personal communication, August 1993), who both came up with this example.

[8] Of course, it is interesting to speculate about why *disgust* is the next best word for *anger,* or *fear* the next best word for *surprise.* Tomkins and McCarter explained this in terms of the amount of overlap in the muscular movements shown in any two expressions. Russell might want to attribute it to similarities in the semantics of the words. These two explanations are not mutually exclusive.

sions but did not let them choose the word that best described each expression. They chose the next best word.

Summary on forced choice. Russell's study, reported in Table 7, does not weaken the universality position. The fact that subjects chose the words most similar to the word they would have chosen if they had been given the choice further strengthens the interpretation that these facial expressions are associated with these particular emotions.

The answer to Russell's criticism of the forced-choice format does not rest on logical analysis alone. There are also decisive new data on this issue, in which subjects were allowed to choose their own word to describe each facial expression. Before I report on that, we need to consider other already published data that did not use forced choice but still found evidence in support of universality.

Quantitative ratings. Russell (1994) said, "Given the problems just mentioned, quantitative ratings on multiple scales are a needed complement to the forced-choice and free-label formats" (p 121). I agree, and we did publish such findings. Although Russell cited our findings, he misunderstood what we did. Although Russell also raised a legitimate criticism of one aspect of our experiment, he failed to report that we replicated these findings in a subsequent experiment that remedied that problem.

Ekman et al. (1987) showed expressions to subjects in 10 literate cultures, asking the subjects to make unipolar ratings from *absent* (0) to *strongly present* (8) on each of seven emotion scales. Russell correctly quoted our finding (on p. 121 of his article) that in 177 out of 180 times, the emotion rated strongest by the largest number of subjects in each culture was the predicted emotion. He then dismissed these findings on two grounds.

First, Russell said that we provided no further detail about these data. He complained that "[they] did not report how they scored ties (Russell, 1994, p. 121). Russell had expected many ties because he mistakenly stated that we had analyzed individual subject's scores, where ties might often occur. We reported that "we determined whether the emotion with the most intense rating was the emotion predicted by Ekman and Friesen and was the same across cultures" (Ekman et al., 1987, p. 715). Because there were three expressions for each of six emotions judged by members of 10 cultures, there were 180 opportunities for the cultures to agree with Ekman and Friesen's predictions and with each other about which emotions are universally signaled by each facial expression. "In 177 out of 180 times, the emotion rated strongest by the largest number of observers in each culture was the predicted emotion" (Ekman et al., 1987, p. 715). Ties did not occur, and thus we did not report how we dealt with them.

The second objection Russell (1994) raised was that our subjects had completed a forced-choice procedure first and then, on a second viewing of the expressions, made the intensity ratings on each emotion: "It is not surprising that the same subject would then give his or her highest quantitative rating to that same emotion term" (p. 122). Although that presumes more memory for past judgments than I think likely, it is a legitimate complaint. What was illegitimate was for Russell to make this objection and then not cite a subsequent study (Matsumoto & Ekman, 1989) that did not suffer from this problem and yet obtained the same findings. Russell should know this study, for he cited it in other regards (p. 109).

Matsumoto and Ekman (1989) obtained the same intensity ratings on each emotion that Ekman et al. (1987) had used, but Matsumoto and Ekman had their Japanese and American subjects do the rating task first, not after making forced-choice judgments. Their results were essentially the same as the Ekman et al. study for anger, disgust, happiness, sadness, and surprise, although there was less agreement among the Japanese for fear.

Russell also failed to mention other findings about universality reported by Ekman et al. (1987). We reported the first evidence of cross-cultural agreement about which of two exemplars of the same emotion is more intense. We also reported cultural differences in the absolute intensity ascribed to particular emotions.

Summary on quantitative ratings. In this section, I showed that Russell's dismissal of the findings using quantitative ratings on multiple scales was unwarranted, based on faulty scholarship. In fact, these replicated findings are consistent with the findings from studies using the forced-choice response format. The case for universality is strengthened because generalizability is demonstrated to a different method—a method which, as Russell says, does not have some of the potential problems found in the forced-choice method. I turn next to yet another method in which subjects are allowed to choose their own words to describe the emotion shown in an expression.

Freely chosen labels. In commenting on the problems he saw with the forced-choice response format, Russell (1994) said, "the question arises of whether this method [forced choice] has created a false impression that most subjects would spontaneously use the same specific emotion label for a given face" (p. 123). Using freely chosen emotion labels is the best method because it imposes no constraints on the subjects beyond having to use words to describe emotions. It has not been used as often as forced choice, because there is no standard, widely accepted method for categorizing such free responses. Without such a standard emotion lexicon, categorizing free responses is an enormous burden. Also, it is hard to evaluate what was found unless the lists of specific words, not just the categorization, are reported.

To use freely chosen labels in a study of different cultures is especially difficult, because it may be very hard to find adequate translations back and forth across every language for all the words subjects in each culture might provide. It has not been easy to obtain adequate translations in every language even for the six or seven emotion words we have used in our studies. Russell might consider that statement an admission that emotions vary across cultures. As I explained earlier, in the section entitled, How Are Words Related to Expressions?, of course they do, and especially in the words used to represent emotions. It is a tribute to the robustness of the phenomenon that despite this variation, it nevertheless is still possible to obtain significant, but not perfect, agreement within and across cultures about certain facial expressions.

I agree with Russell that neither Izard (1971), nor Boucher and Carlson (1980), who did use free responses, provided enough information about how they categorized the words. For that reason, I won't consider their findings further, except to point out that they did find significant agreement. But that is not the issue for Russell. Instead Russell emphasized that the free-label results did not produce as much agreement as the forced-choice response format. I readily concede that point; there is no reason to expect otherwise. (See Izard's, this issue [pp. 295–296], discussion of this point.)

Freely chosen labels allow subjects to use a variety of words that reflect different aspects of an emotion, for example, antecedents and physical sensations. It is only someone who maintains that there is but one word for each emotion, who believes that this one word has

no synonyms, who denies there are words that denote variations in intensity, and who admits no emotion metaphors—it is only such a straw man who would be surprised that agreement was lower with freely chosen labels than with a forced-choice format.[9] I cannot believe that Russell holds that position. The question with freely chosen labels—as it was with the forced-choice results—is whether agreement is better than chance. It was.

I discuss only one of Russell's own studies using freely chosen labels, because the point I make about it applies to his other studies as well. Russell's (1994) Table 10 reports responses when subjects were shown an anger expression and asked, "What mood or emotion is the woman [man] in the photograph feeling?" (Russell, 1991a, p. 284). For Russell (1994), the point of Table 10 was to show how much disagreement there was when subjects were allowed to choose freely their own label. The clearest case for Russell was that 30.6% of the subjects gave the word *frustration,* which was more than the 25.6% who gave the word *anger.* If *frustration, anger, irritable, mad* and *pissed off* are considered to be members of the anger family of words, then the data in Russell's Table 10 yield 70.9% agreement in the judgment of an anger expression. Russell argues, however, that there is a problem with including the word *frustration* in the anger category. *Frustration* refers, he says, to a situation rather than an emotion.

I consider *frustration* as an antecedent event that commonly calls forth anger. Mine is not a novel position. It is consistent with the theoretical positions or empirical data from Arnold and Gasson (1968), Ellsworth and Smith (1988), Roseman (1984), Scherer (1993), and Weiner (1985), not to mention Dollard, Doob, Miller, Mowrer, and Sears (1939). Is there something wrong with including in the concept of anger not only general descriptive terms such as *anger, mad,* or *irritable* but also terms that describe the antecedent event?

Russell apparently considers antecedent events and emotion labels to be different phenomena, and it is only the labels that are to him relevant to the question of whether expressions are associated with emotions. I assume that he takes this position because he is considering his circumplex model of how labels are used to judge emotion rather than a general theory of emotion. As I explained earlier, in the section entitled How Are Words Related to Expressions?, that is not my position. For me, emotion labels are a shorthand that refers to any of a number of different aspects of an emotion—including antecedents, expressions, memories, and consequences.

More definitive evidence on freely chosen labels comes from a study by Rosenberg and Ekman (1993) that allowed subjects to choose their own emotion labels. We selected two expressions from the Matsumoto and Ekman (1988) set that best met FACS scoring procedures for each of seven emotions.[10] These facial expressions were shown to 35 college students. Their instructions were the following:

> Your task in this experiment is to look at the facial expression shown in each slide and make a judgment about how the person in the slide feels. In each blank space listed below, write the one word that you think best describes how the person feels. (Rosenberg & Ekman, 1993)

The subjects' responses were categorized by means of the Human Interaction Laboratory Emotion Lexicon (HELEX; Ekman & Irwin, 1993). HELEX is a database of 820 root words (1,087 total words) classified into 20 categories (emotion and nonemotion categories, including all of the categories that have been used in forced-choice paradigms).

Table 3 contains binomial tests of whether agreement was better than chance. Because investigators may differ in what they consider chance levels to be in this kind of task, Table 3 contains significance tests using different chance levels. We set chance at 1/5 for the five negative emotions. We also used a more conservative chance level of 1/3 for anger, disgust, and contempt, which are often confused with each other but not with fear or sadness. For surprise, we set chance at 1/3, because it is sometimes confused with fear or happiness. We also used a more conservative chance level of 1/2 for fear and surprise, because they share many morphological characteristics. For happiness, which was the only positive emotion, we set chance at 1/2. A significant level of agreement was found. Although agreement for disgust was lower than that for the other expressions, it was still significant. Table 4 contains the exact words that HELEX categorized into each emotion category, so the reader can see the basis for the classification used.

Summary on freely chosen labels. When subjects freely chose their own labels, a statistically significant level of agreement was still obtained. It is only if we consider single emotion labels to be sacrosanct, and do not include antecedent events such as frustration, that doubts can be raised about the Rosenberg and Ekman findings reported in Tables 3 and 4. Even if we were to grant that, it would only raise question about the findings on anger, not the other five emotions. These results still might not persuade Russell, however. He might repeat one of the objections he made in his article—that subjects were asked to describe how the person feels rather than to just describe the person without any mention they should consider feelings or emotions. I cannot consider that a serious problem.

These findings with freely chosen labels were gathered only in one culture, of course, not across cultures. They do, however, directly answer Russell's question about whether the forced-choice method "has created a false impression that most subjects would spontaneously use the same specific emotion label for a given face" (Russell, 1994, p. 123). There is no reason to think that the same answer to Russell's question would not be obtained across cultures if the Rosenberg and Ekman (1993) study was repeated in different languages. The only exception might be in those languages that have no words for a particular emotion. The Dani of West Irian are a culture whose language has no words for most of the emotions. Later, I describe how evidence of universality in facial expressions of emotion was found with those people.

Other Issues With the Literate Culture Data

Subjects. Russell presented Ducci, Arcuri, W/Georgis, and Sineshaw's (1982) finding of lower agreement among rural than

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[9] Russell might think that I took that position because of what we wrote (Ekman & Friesen, 1988) in reply to Izard and Haynes (1988). My point then was not that Izard and Haynes had used more than one word, but that there was no independent evidence that the words Izard and Haynes included were all part of the contempt family. More recent work on the language of emotion (Ekman & Irwin, 1993) suggests that Izard and Haynes may have been correct.

[10] We do not report here the findings on contempt, which are more complicated but do support the association between that expression and that emotion.

Table 3
Percentage Agreement of Free-Response Categories With Predicted Emotions for Six Facial Expressions

Facial expression	%	Chance criterion 1	Chance criterion 2
Anger	85.7[a,b]	.20	.33
Disgust	57.1[a,c]	.20	.33
Fear	72.8[a,d]	.20	.50
Happiness	94.3[a]	.50	NA
Sadness	75.7[a]	.50	NA
Surprise	74.3[a,c]	.33	.50

Note. $n = 35$. Criterion 1: For all of the clearly negative emotions (anger, contempt, disgust, fear, and sadness), chance was the probability of any one of them being chosen: 1/5 or .20. For happiness, the only positive emotion in the list, the most conservative chance criterion is the choice of a positive versus a negative expression, which is .50. In the case of surprise, a chance criterion of 1/3 was used because subjects sometimes report happiness or fear. Criterion 2: Because anger, contempt, and disgust can be confused with each other, chance was set at 1/3. Because fear and surprise share morphological characteristics (raised eyebrows and wide eyes), the chance criterion for these expressions was set at 1/2. There was no second criterion for sadness because there are no clear options for other facial expressions with which it is confused. The chance criterion for happiness could not be made more stringent; therefore, a second criterion was not derived.
[a] Different from Chance Criterion 1, $p < .001$. [b] Different from Chance Criterion 2, $p < .001$. [c] Different from Chance Criterion 2, $p < .01$. [d] Different from Chance Criterion 2, $p < .05$.

urban Ethiopians in judging expressions, as reason to question universality. The issue, of course, is not whether rural agreement was lower than urban agreement, but whether there was significant agreement among the more isolated rural subjects. The rural subjects reached significant agreement in their judgments of anger, fear, sadness, surprise, happiness, and disgust; the only exception was contempt. Russell omitted Ducci et al.'s interpretation of their findings: "This supports the view that six of the seven emotions displayed are universally recognized" (Ducci et al., 1982, p. 347). Russell next raised the problem that perhaps the college students who had participated in some of the literate culture studies may have learned the facial expressions by reading about the widely heralded universality findings in their textbooks. This is another red herring. Universality findings did not reach textbooks until the middle 1970s. Results reported by our research group and by Izard, which were gathered in the 1960s, obtained just as much agreement as the results obtained in the 1980s, a fact that Russell acknowledges.

Presentation of stimuli. Russell said it was problematic that three investigators showed their subjects all or some of the expressions before asking them to make judgments. Yet in one of those studies (Boucher & Carlson, 1980), the level of agreement was not significantly different than in studies in which there was no previewing. In Winkelmayer, Exline, Gotthiel, and Paredes's (1978) study, which did report a difference in the level of agreement, there were too many other differences in the design to attribute it to previewing. The third study that Russell cited for using previewing was done by Izard (1971), but he did not use that procedure.

Next, Russell claimed that allowing subjects to see and judge multiple expressions was a problem because the subjects might have compared the expression to be judged with their recent memory of other expressions. But there is no compelling reason to prevent subjects from making such comparisons unless one were to maintain that in life each expression is seen totally out of the context of any other expression by that or any other person. Only a person who had but one glimpse of one expression on the face of one person before becoming permanently blind would have such limited experience.

Russell's last objection in this section is that the order of presentation of the faces was the same across subjects. However, that was only so within each experiment. Russell reviewed eight different literate culture experiments, and each used different orders and obtained similar findings.

Facial expressions. Russell complained that we and others preselected our expressions. This complaint is based on his misunderstanding of what is meant by universality. The neuro-cultural theory of facial expression does *not* maintain that every expression will be universally understood. We had theoretical and empirical reason to expect that certain expressions would be universal, and of course, we selected just those stimuli. Russell cited two studies (Malatesta, Fiore, & Messina, 1987; Winkelmayer et al., 1978) that did *not* select expressions, but instead obtained judgments on all of the expressions that their subjects had posed. Russell noted that agreement in these studies was lower than it was in my studies or Izard's, in which we had preselected the expressions that were shown. It should have been; a different question was being asked. By obtaining judgments on all the poses, Winkelmayer et al. (1978) and Malatesta et al. (1987) provided information on how well their subjects could pose. Such studies answer the following question: What is the level of agreement in labeling a representative or complete sample of poses? By showing only expressions selected according to an a priori criterion to people from different cultures, we determined whether *those* expressions were interpreted the same way across cultures. Ours was not a study of posing, but a test of the

Table 4
Percentage of Subjects Who Gave Each Word Classified by HELEX

Word	%	Word	%
Anger		Happiness	
Angry	25.8	Happy	77.1
Anger	15.7	Happiness	11.4
Mad	18.6	Pleased	2.9
Frustrated	8.6		
Frustration	5.7		
Furious	4.3	Sadness	
Fury	2.9	Sad	42.9
Disgust		Sadness	12.9
Disgust	34.3	Depressed	4.3
Disgusted	17.1	Disappointed	4.3
Grossed-out	2.9	Disappointment	2.9
Fear		Surprise	
Fear	28.6	Surprise	35.7
Scared	27.1	Surprised	24.3
Frightened	4.3	Amazed	4.3
Afraid	2.9	Astonished	2.9
Fright	2.9		

Note. HELEX = Human Interaction Laboratory Emotion Lexicon. Words given by only one observer not reported.

universality of certain specific facial expressions. (See Izard, this issue, p. 291, on this point.)

Posed versus spontaneous facial expressions. Russell (1994) said that most of the literate culture studies used posed facial expressions and therefore there might be a problem of ecological validity. Russell misled the reader about what we actually did. We did not simply use the facial expressions that someone had posed. We picked our expressions in a very different way, and I was very explicit about why and how we did this.

> The most crucial aspect of the design of this experiment [in literate cultures] is how the sample of faces to be shown to observers across cultures was selected. The typical procedures utilized by other investigators were not appropriate for our purposes. Sampling pictures on the basis of the actor's intended pose (i.e., Triandis & Lambert, 1958), or showing poses which had elicited high agreement within one culture to members of another culture (i.e., Izard, 1968) would be vulnerable to the inclusion of facial expressions which were culture-specific . . . [I explained how this could occur, and then said if we were to avoid culture-specific expressions] . . . we needed some other means for deciding whether a face showed emotion at all, and if it showed only one emotion. We selected faces on the basis of descriptions of the facial configurations which distinguish among emotions, concurrently being developed by Ekman, Friesen and Tomkins for their Facial Affect Scoring Technique. . . . Selection, then, was done by applying to each face [we inspected] a *theoretically* based set of criteria, which specified the presumed appearance of each emotion. (Ekman, 1972, pp 261–262, italics in the original)

Although many of the photographs we used originally had been posed, not all were, and it is misleading to leave it at that, implying that it was posing, not the presence of a priori specified facial configurations, which was our basis for selecting expressions to study. About half of the studies that Russell summarizes in his Table 2 studied facial expressions selected according to a priori theory, and they did not obtain any less agreement than the other studies that selected expressions by some other means.

Russell (1994) went on to discuss three studies of spontaneous facial expression, but two of them did not include any cross-cultural comparisons (Motley & Camden, 1988; Wagner, MacDonald, & Manstead, 1986). These two studies obtained contradictory results, and each had flaws in research design.[11] The third study of spontaneous behavior that Russell discussed did include a cross-cultural comparison; it was my own (Ekman, 1972). Russell described only my first experiment, but he failed to report all of those findings. Russell failed to describe most of the findings from my second experiment, which produced unique and important findings in support of universality.

Russell described my first experiment as follows:

> Emotion-inducing films were shown to 25 Japanese and 25 American viewers, whose faces were secretly videotaped. . . . Two segments from each videotape, one taken while the viewer watched a "neutral" travelogue and the other taken while the viewer watched a "stress" film (industrial accidents and the like), were then shown to a sample of judges. Judges were asked to guess whether the viewer was watching the travelogue or the stress film. According to Ekman (1972), "about 60% of the judgments" (p. 243) were correct (chance would have yielded 50%). (Russell, 1994, p. 114)

Russell omitted our overall finding that supported universality: There were no differences in accuracy as a function of whether the Japanese or Americans were judging members of their own or the other culture. Russell also failed to report the further tests we made to establish universality. I now quote from my report:

> Since only 60% of the judgments by the observers in either culture were correct the possibility remained that the observers from the two cultures were correctly judging different stimulus persons. . . . The test of this culture-specific hypothesis was to correlate the proportion of correct responses by the Japanese and the American observers on each subject [stimulus person]. This correlation, which is also the best statistical test of our hypothesis of universal facial expressions, was made to determine if those who were judged correctly by one culture were also judged correctly by the other culture. . . . The correlations were both positive and high. The correlations (Pearson product moment correlations) between Japanese and American observers' judgments of the United States subjects' facial expressions was .86, and between their judgments of the Japanese subjects' facial expressions was .79. (Ekman, 1972, p. 243)

I went on to report also a replication with new set of spontaneous facial expressions in each culture and new observers in each culture.

Russell criticized this experiment for not proving that subjects in each culture agreed in interpreting facial expressions as showing specific emotions (e.g., disgust or fear) Of course, that is true, as I noted in my discussion of the findings. Other important evidence was available in a second experiment, which found that the same specific facial expressions of emotions were shown by the Japanese and American subjects, but Russell gave little attention to those findings because they were not based on how subjects judged the emotions shown in facial expressions. Instead I directly measured the facial movements shown by the Japanese and American subjects who had watched the stress-inducing films.

I quote now from my discussion of both the *first* experiment (in which Japanese and American observers judged whether the spontaneous facial expressions shown by other American and Japanese subjects had been in response to watching an unpleasant or pleasant film) and the *second* experiment (in which I measured the facial movements that the Japanese and American subjects had shown):

> The first experiment disproved the notion that facial expressions are culture-specific in the sense that members of each culture can only

[11] Motley and Camden (1988) did not obtain significant levels of agreement when subjects judged six different spontaneous emotions induced experimentally. Two problems raise doubt about the validity of this study. First, they used a still photograph to represent a spontaneous emotional expression. While stills have some use for poses, because poses typically are held for a number of seconds, spontaneous behavior is rarely so frozen, and a still photograph may fail to adequately represent the flow of motion. Second, there was no adequate check for whether the induced emotions actually occurred, not even systematic self-report data. Measurement of galvanic skin responses did suggest that some emotion occurred, but not which one. Wagner, MacDonald, and Manstead (1986) at least used videotape, not still photographs, to record the spontaneous facial expressions they studied. While they interpreted their findings as showing significant agreement for disgust, anger, surprise, and happiness, Russell (1994) questioned how significant those findings actually were. I question whether we should interpret these findings at all. Sometimes emotions occur without any observable facial expression (Ekman, 1993). Wagner et al. did not measure the facial activity in the videotapes they showed to their subjects, and so we cannot know just how many of the subjects on the videotapes showed any visible facial behavior at all.

> accurately recognize the reactions of members of their own culture. Instead the first experiment showed that the facial expressions of Japanese and Americans have a similar meaning to Japanese and American observers. But that experiment studied only the judgment of facial expression and could not tell us if the actual facial expressions shown by the Japanese and American subjects were similar. This experiment [in which we measured the actual facial behavior] has answered that question. We have found great similarity in the facial expressions shown during the stress films in both of these cultures. Whether measurements of separate facial areas or of combined activity of the total face were considered, and whether the measurements were considered on the level of specific behavioral description or integrated into emotion categories on the basis of theory, the results were the same: strikingly similar facial responses in these two cultures. (Ekman, 1972, p. 259)

This is important evidence of universality. It is important because a different method was used—directly scoring facial muscular movements—than the method used in all the other literate culture studies, in which observers made inferences about what emotion was shown in an expression. It is also important because it is the *only* study in which *spontaneous* facial behavior shown by members of two cultures was directly measured. In support of universality, this study found that the specific facial actions that signal the emotions of fear, sadness, disgust, surprise, and happiness occurred with virtually the same frequency by Japanese and American subjects.

> The same types of [spontaneous] facial expressions of emotion were shown by about the same number of people in both [Japanese and American] cultures. The [rank order] correlation between cultures calculated on these figures is extraordinarily high (.97). (Ekman, 1972, p. 259)

Russell's only mention of this finding is

> specific facial movements such as smiling or lowering the brow were found on the videotapes in similar frequency in the two [Japanese and American] samples. (Russell, 1994, p. 114)

Overall Summary on Literate Culture Studies

Russell's (1994) criticisms of the literate culture data are not relevant to the crucial issue, which is whether significant agreement was obtained across cultures in the interpretation of facial expressions of emotion. Acknowledging that statistically significant agreement was indeed found, Russell tried to discredit those findings by raising almost a dozen possible problems. To answer Russell's criticisms of the response format used in the literate culture studies, I presented new data, published data that Russell omitted, and reanalyzed Russell's own findings.

Significant agreement in the judgment of facial expressions was found with two response formats less widely used than the forced-choice format: (a) quantitative ratings on each of six or seven unipolar emotion-rating scales and (b) free response, in which the subjects gave their own emotion label. The findings with quantitative ratings are entirely consistent with the large number of cross-cultural studies of literate cultures that used a forced-choice format. The free-choice findings provide the basis for dismissing Russell's challenge that very different results would be obtained if subjects could choose their own terms rather than being restricted to a forced-choice format.

My review of the other doubts Russell raised about the literate culture studies—regarding the subjects, the presentation of stimuli, and the facial expressions—showed that they have no merit. Findings in which facial behavior itself was measured, rather than measuring observers' judgments of faces, provided consistent evidence in support of universality, and this study examined spontaneously occurring facial behavior. One more legitimate question remains about the literate culture studies. It is not about whether there was significant agreement, but how to explain that agreement. I turn next to a consideration of the evidence from preliterate cultures that is relevant to that issue.

Does the Preliterate Culture Data Support Universality?

It is possible that subjects in all of the literate cultures who were studied, including the non-Western cultures, learned these emotional facial expressions from intercultural contact or from a common source, such as movies or television. If that is so, then people without such shared input should interpret facial expressions quite differently than did the literate culture subjects. This was the argument made by the cultural relativist Birdwhistell (personal communication, March 1967) to challenge my claim that our findings on literate cultures indicated there are some universal facial expressions of emotion. It was to answer Birdwhistell's argument that I made two trips to New Guinea, in 1967 and again in 1968.

In addition to my own research in New Guinea, Russell also discussed Sorenson's (1975, 1976) study of isolated preliterate cultures, Heider and Heider's (reported in Ekman, 1972) study of a very isolated preliterate culture, and Boucher and Carlson's study of the Temuans. I do not answer Russell's criticisms of Boucher and Carlson's (1980) study because their findings are not as crucial. Although Boucher and Carlson did find evidence in support of universality, the Temuans were not nearly as isolated as the other cultures that I discuss.

Sorenson's Study of the South Fore, the Bahinemo, and the Sadong

Russell's report of this work quotes Sorenson's (1976) account of the difficulties he encountered working in a preliterate culture. Russell did not report that Sorenson (1975) said that he had overcome most of those difficulties. The most important omission in Russell's treatment of Sorenson is the failure to report that Sorenson concluded that he had found evidence of universality.

Sorenson and I worked together in 1967 during my first trip to New Guinea. He did not participate in the research I did during my second trip. Sorenson reported (1975, 1976) the use of two methods in his research. He used freely chosen labels with the South Fore of New Guinea; the Bahinemo, who are from a very different region of New Guinea; and the Sadong from Borneo. With the South Fore, Sorenson (1975, 1976) also used a method he borrowed from us (Ekman & Friesen, 1971), in which subjects are asked to select the expression that fit a story.

Sorenson preferred the freely chosen labels methodology to the story method, because he thought it was less subject to error. However, Sorenson was not fluent in the languages of any of the people he studied. He did not report how he verified whether his translations of their free responses were correct. He also did not report how he grouped their various emotion labels into the emotion

categories that he did report. I will postpone discussion of Sorenson's findings with the story method until I describe our own work with it.

Although Sorenson also studied the South Fore who had considerable contact with Westerners, it is his findings on the South Fore who had very little contact with Westerners that is most relevant to questions that I consider. Sorenson's report suggests that these South Fore subjects agreed with how literate culture subjects interpret happiness and anger expressions. However, he did not report any statistical tests, and I could not now compute any tests because Sorenson did not provide data on the size of this sample.

It is amazing that Sorenson found any evidence of agreement with freely chosen labels, given the difficulties he encountered testing these subjects. I quote Sorenson: "Some were completely tongue-tied; others trembled and perspired profusely, or looked wildly about. The least acculturated were most afflicted; they often seemed bewildered, even fearful" (Sorenson, 1975, p. 366).

Sorenson did provide information about the size of his Sadong sample, so I was able to compute binomial tests using as chance expectations .50 for happiness, .33 for surprise, and .20 for the five negative emotions he studied. Better than chance agreement was evident for happiness ($p < .01$), for anger ($p < .01$) and for sadness ($p < .05$).

The Bahinemo gave no differentiated responses at all, and Sorenson reported no numerical data for them. He did say, "Also, unlike the Fore, there was a reticence associated with socioideological secrecy as means of maintaining group solidarity" (Sorenson, 1975, p. 363). Here is Sorenson's interpretation of his failure to obtain any results with this group (not described by Russell). Sorenson explicitly said it is

> not a challenge to this view [universality]. . . . The difficulty here seems to have stemmed primarily from using pictures of Caucasians, although use of interrogative procedures unfamiliar to the Bahinemo and the unavailability of skilled translator–assistants may have contributed to the indeterminacy of these results. (Sorenson, 1975, p. 371)

Russell also did not report the conclusion Sorenson drew about what he had found across all of his studies of preliterate people. In concluding his report, Sorenson's last two sentences stated the following:

> Clearly, there are universals in the facial expressions of emotion; but, just as clearly, culture and social organization may pattern the expression of these universals. Further work is required before a more specific definition of the limits of universality can be developed. (Sorenson, 1975 p. 371)

I agree with this statement completely.

Ekman and Friesen's Study of the South Fore

I focus on our findings on those subjects who had the least contact with Westerners. I fully describe what we did and show why Russell's many objections to our methods and analyses should be dismissed. I consider this work in such detail because it was crucial in showing that shared visual contact was not necessary to our finding of universality. This study removed the greatest challenge to the universality findings established by the literate culture studies. I provide detailed information about this New Guinea study also because it has been cited so often by others over the last 20 years. It is important that this study be understood because it is doubtful that such work can any longer be done. It is unlikely there are any more visually isolated preliterate cultures. First an aside: Russell (1994) said "it is especially unfortunate that these studies have not been published in full" (p. 127). The findings from my first trip to New Guinea were published in *Science* (Ekman, Sorenson, & Friesen, 1969). The findings from my second trip to New Guinea were reported in an article devoted just to it in the *Journal of Personality and Social Psychology* (Ekman & Friesen, 1971). I also discussed our research in New Guinea in seven pages of my chapter in the *Nebraska Symposium on Motivation* (1972) and in five pages in a chapter that I wrote reviewing cross-cultural studies over a 50-year period (Ekman, 1973). Perhaps Russell wants me to report findings from the pilot studies I did during both trips, trying out methods to see what might be most suitable for subjects who had no written language and who had never seen a photograph. It is not standard practice to report pilot studies in any detail. We did publish how those pilot studies led us to adopt the methods we used in our full study. I quote that description in the paragraph after next.

I did not use freely chosen labels in any study, for that would have required a complete mastery of the language of the South Fore people, which neither I nor Sorenson had. In my initial study in 1967, when I worked with Sorenson (Ekman et al., 1969), we used a forced-choice response format. We obtained statistically significant agreement from those South Fore who had the most contact with Westerners. But this method was not suitable for those who had the least contact.

We (Ekman & Friesen, 1971) wrote the following:

> The least Westernized subjects could not be asked to select from a printed list of emotion terms the one that was appropriate for a photograph, since they could not read. When the list was repeated to them with each photograph, they seemed to have difficulty remembering the list. Further, doubts remained about whether the meaning of a particular emotion concept was adequately conveyed by translating a single English word into a single South Fore word. Asking the subject to make up his own story about the emotions shown in a picture was not much more successful, although the problems were different. Subjects regarded this as a very difficult task, repeated probes were necessary, and as the procedure became lengthy, subjects became reluctant.
>
> To solve these problems, it was decided to employ a task similar to that developed by Dashiell (1927) for use with young children. Dashiell showed the child a group of three pictures simultaneously, read a story and told the child to point to the picture in which the person's face showed the emotion described in the story. The advantages of this judgment task in a preliterate culture are that: (a) the translator recounts well-rehearsed stories which can be recorded and checked for accurate translation; (b) the task involves no reading [by the subject]; (c) the subject doesn't have to remember a list of emotion terms; (d) the subject need not speak, but can point to give his answer; and (e) perfect translation of emotion words is not required since the story can help provide connotations.
>
> With the exception of the stories for fear and surprise, those used in the present study were selected from those which had been most frequently given in the pilot study. Considerable care was taken to insure that each story selected was relevant to only one emotion within the Fore culture, and that members of the culture were agreed on what that emotion was. Since the stories told by the pilot subjects for fear and surprise did not meet these criteria, the authors composed stories for these emotions

> based on their experience within the culture. (Ekman & Friesen, 1971, pp. 125–126)

Five researchers gathered the data among the South Fore using the antecedent story method in 1968. Three were men (myself, Friesen, and Neville Hoffman, an Australian physician who had earlier lived for 2 years among the South Fore and who was well liked and well known), and two women (Hoffman's wife and my wife). The women worked with female adults and children, the men with male subjects. I usually did not collect data but floated among the other research teams to deal with any logistical problems. Friesen and Hoffman alternatively operated as the primary male data collectors. Except for Hoffman, who was sufficiently proficient in the South Fore language, each researcher was accompanied by a South Fore assistant, whose sole job was to explain the task.

In Table 5, I reprint the published table of our results from the adult subjects, so that the reader can see what we found and the amount of detail we provided about the different emotion discriminations these subjects were able to make. (The findings with children were consistent with the adult findings.)

> A binomial test of significance assuming chance performance to be one in three showed that the correct face was chosen at a significant level for all of the discriminations (rows) except that of fear from surprise. (Ekman & Friesen, 1971, p. 127)

Table 5
Judgments by Visually Isolated New Guineans

Emotion described in the story	Emotion shown in the two incorrect photographs	*N*	% choosing correct face
Happiness	Surprise, disgust	62	90**
	Surprise, sadness	57	93**
	Fear, anger	65	86**
	Disgust, anger	36	100**
Anger	Sadness, surprise	66	82**
	Disgust, surprise	31	87**
	Fear, sadness	31	87**
Sadness	Anger, fear	64	81**
	Anger, surprise	26	81**
	Anger, happiness	31	87**
	Anger, disgust	35	69*
	Disgust, surprise	35	77**
Disgust (smell story)	Sadness, surprise	65	77**
Disgust (dislike story)	Sadness, surprise	36	89**
Surprise	Fear, disgust	31	71*
	Happiness, anger	31	65*
Fear	Anger, disgust	92	64**
	Sadness, disgust	31	87**
	Anger, happiness	35	86**
	Disgust, happiness	26	85**
	Surprise, happiness	65	48
	Surprise, disgust	31	52
	Surprise, sadness	57	28[a]

Note. From "Constants Across Cultures in the Face and Emotion" by P. Ekman and W. V. Friesen, 1971, *Journal of Personality and Social Psychology, 17*, p. 127. Copyright 1971 by the American Psychological Association. Adapted by permission.
[a] Subjects selected the surprise face (67%) at a significant level ($p < .01$, two-tailed test).
* $p < .05$. ** $p < .01$.

Russell criticized us for not recognizing that when subjects are provided with three choices, one of the choices might be very easy to rule out, and therefore chance might not be 1 in 3 but 1 in 2:

> If a sad story was given to a Fore adult, for example, and the choice was among three options, a "sad," an "angry," and a "happy" facial expression, then 50% recognition would be achieved by distinguishing positive from negative expressions (eliminating the "happy" expression and choosing randomly between the other two). (Russell, 1994, p. 127)

Russell's criticism is unfair and misleading. We had thought of this problem, and we did report that we used not only a 1 in 3 but also a 1 in 2 chance expectation in our computations. Russell had to know that we had done what he told the reader we had not done, for it was clearly described in the very article Russell discussed. Four lines after we reported the binomial tests assuming chance to be 1 in 3, which I quoted above, we wrote the following:

> [A] binomial test assuming chance to be one in two (a more conservative test, justified if it was thought that within a set of three pictures, there may have been one which was obviously wrong) still yielded significant correct choices for all but the fear from surprise discriminations. [And, there was] . . . no significant difference between the most and least Westernized subjects. (Ekman & Friesen, 1971, p 127)

Russell (1994) said that our findings should not be considered relevant to emotion because of the way in which we had derived our antecedent stories. Findings with the antecedent story method, he says, should be considered relevant to emotion only "if these stories captured pancultural antecedents of emotion, as specified a priori by the experimenter's theory" (Russell, 1994, p. 128). Although it is true that we did not develop those stories on the basis of a priori theory, it is surprising that Russell did not note that the antecedent stories that we used were consistent with empirical findings later published about pancultural emotion antecedents (Boucher & Brandt, 1981; Scherer, Wallbott, & Summerfield, 1986). Our antecedent stories are also consistent with theory published later by Lazarus (1991).

Russell also objected that "the story and the emotion were confounded. For happiness, it is unclear whether the Fore or the Dani subject [he refers to Heider's data, which I discuss below] selected the smile as indicating happiness or as the response to meeting a friend" (Russell, 1994, p. 127). I believe that for the issue of universality, it does not matter which they selected the smile as indicating. It is not just emotion labels that should be our proper concern. It is just as relevant and just as important from my theoretical position to find that an expression is linked to a universal antecedent (to be more precise, to an event that is very likely to be appraised in the same way by most or all people) as it is to show it is consistently linked to a particular emotion label:

> If emotions are viewed as having evolved to deal with fundamental life-tasks in ways which have been adaptive phylogenetically, then it is logically consistent to expect that there will be some common elements in the contexts in which emotions are found to occur. This is not to presume that every social context which calls forth an emotion will be the same for all people within or across cultures. Clearly there must be major differences attributable to social learning experiences. . . . My view on this matter . . . is in agreement with Ohman [1986], Lazarus [1991], Johnson-Laird and Oatley [1989], and Stein and her colleagues [Stein & Levine, 1989]. (Ekman, 1992, p. 184)

Russell described numerous other problems that *might* have occurred, which if they had occurred, could have rendered our findings untrustworthy. He then acknowledged that he engaged in what he terms a "fictional reconstruction" and that "no evidence shows that anything like this fictional reconstruction actually occurred" (Russell, 1994, p. 127). Science is not a fictional enterprise. Research reports should not be judged by fictional reconstructions.

Investigators must, of course, attempt to anticipate problems that could confound or contaminate their findings. They should take steps to deal with those problems and report those steps so the reader can evaluate whether their findings might be due to artifacts. We recognized that many problems could occur in our work in a preliterate culture. We took a number of steps to deal with these potential problems, and we reported those steps. Russell did not inform the reader of the precautions we reported that we took to diminish problems that could have invalidated our findings.

We wrote about this as follows:

> Considerable practice and explanation was given to the translators. They were told that there was no correct response and were discouraged from prompting. Repeated practice was given to insure that the translators always repeated the stories in the same way and resisted the temptation to embellish. Spot checks with tape recordings and back translations verified that this was successful. The Caucasians, who did not know the correct responses, averted their faces from the view of the subject, looking down at their recording booklet, to reduce the probability of an unwitting bias effect. Data analysis did not reveal any systematic differences in the responses obtained with different translators. (Ekman & Friesen, 1971, p. 126)

Let us turn now to Sorenson's use of the antecedent story method with the South Fore. Sorenson's findings, using this method with the South Fore who had the least contact with Westerners, are virtually identical to our findings. Sorenson did not compute any statistical tests, but when we computed binomial tests setting the probability of obtaining the findings by chance at .50, we found that there had been significant agreement ($p < .001$) for the judgment of anger, disgust (not separated from contempt), fear, happy, and sad expressions. The figures listed in Sorenson's table are within a percentage point or two of the figures from our findings listed in Table 5.

How should we regard Sorenson's account of the difficulties he encountered in "controlling the testing situation" (Sorenson, 1975, p. 364) with the antecedent story method? Russell presented a full account of Sorenson's belief that subjects might have cheated and the experimenters or translators might have leaked the expected response when this method was used. Russell used Sorenson's worries about this to cast doubt not only about Sorenson's findings but also about my data. But Sorenson could not have known if there were any such errors in my data collection.

Russell must have presumed that Sorenson had participated in our data collection with the antecedent story method, but Sorenson was not part of the research team I assembled to collect the data with the antecedent story method.[12] Sorenson was describing his own problems in using this method—problems Sorenson encountered, in all likelihood, because I had not taught him how to use the precautions I described earlier to reduce or eliminate errors in administering this task. I did train the Heiders in how to use the antecedent story method, and they used the method without having any of the problems encountered by Sorenson in another visually isolated culture. Let us turn to their work.

The Heiders' Study of the Dani

This is a crucial study. The Dani were more isolated than the South Fore. More importantly, Karl and Eleanor Heider [now Rosch] undertook this study because Karl Heider thought we were wrong about universals. Russell did not let the reader know this. Russell never told the reader who collected the Dani data; by implication it was me, not the Heiders. I described the Heiders' motives in reports Russell cited in other regards:

> Karl and Eleanor Heider [Rosch], an anthropologist and a psychologist, were skeptical of our claim that at least some facial expressions of emotion are universal. . . . [They]. . . were doubtful that the Dani people would judge our photographs of facial expressions of emotion in the same way as did members of the other cultures, particularly in view of the fact that the Dani do not have words for all six emotions studied. (Ekman, 1973, p. 214)

Although Heiders' results were not separately reported, I (Ekman, 1972) did report fully their quantitative findings.

The Dani results were nearly identical with the South Fore. Among the Dani, there was significant agreement for happiness, sadness, disgust, and surprise. Although fear was discriminated from the anger, sad, and disgust faces, the results discriminating fear from surprise were not consistent. The Dani, unlike the South Fore, did not discriminate anger from disgust, although they did discriminate anger from happiness, sadness, and fear. Having spent nearly 3 years studying Dani culture, K. Heider knew enough about this culture to be able to interpret their failure to distinguish anger from disgust. He said (Heider, 1991a) that this failure is central to Dani attitudes about anger. It was not an error, but an instance of an interpretable culture-specific difference.

Summary

In studies of subjects from a number of quite different preliterate cultures, all of whom had minimal contact with outside cultures, consistent evidence was obtained of agreement in the judgment of many of the facial expressions of emotion. There were six studies of subjects with minimal contact: Sorenson's Sadong and Bahinemo samples, two different South Fore samples studied by Sorenson, a South Fore sample studied by Ekman & Friesen, and the Heiders' Dani sample. Two different methods were used: freely choseh emotion labels (Sorenson), and choose the expression that fits an antecedent story (Sorenson, Ekman, & Friesen, and Heider & Heider).

Significant agreement for at least *some* facial expressions of emotion was obtained in five of the six studies. The exception was the Bahinemo, who did not give differentiated responses to any expressions. However, Sorenson judged them to be unable or unwilling to cooperate with the task. In two of the five remaining studies,

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[12] Whereas Sorenson was a coauthor of my first experiment with the most Westernized New Guineans, in our second experiment, in which we used the antecedent story method, Sorenson was not a coauthor. In our publication of our study with the antecedent story method, we did thank Sorenson in a footnote but only for his previous " collaboration in planning the early stages of this research, and providing background information" (Ekman & Friesen, 1971).

Sorenson's free-label data with the South Fore and the Sadong, the distinctions that reached significance were limited: happiness and anger, in both of these cultures; and also sadness in the Sadong. However, those studies required that the subjects provide their own words, and Sorenson was not fluent in the Fore language, and it appears from his report, that he did not know the Sadong language at all. Sorenson did not explain how he verified the accuracy of his translations nor how he grouped the translated words into categories. He did report that his subjects had a great deal of difficulty freely choosing their emotion labels.

In the three studies that did not require that the subjects freely provide verbal labels, bypassing potential sources of error in translation and categorization, much stronger evidence was obtained. Statistically significant agreement was obtained for five to six emotions in each of those three studies. There were, of course, also variations in what was found, which may be due to difference in methods, to genuine cultural differences in the representation of emotion, or to error.

The possibility that there are no universals in facial expressions, that common expressions occur only when people have observed each other or the media, surely can be ruled out. Russell's criticisms of these studies are unfounded or based on imagination, not the reality of what was done nor on what was found.

In criticizing these studies, Russell set a standard by which all research done outside of the confines of laboratory would be discredited: "These experiments were conducted in the field rather than in a laboratory, and it is unlikely that the experimenters were able to control precisely the information given to the subject" (Russell, 1994, p. 127). So much for any attempt to study visually isolated people, or for attempts to obtain ecological validity by using field as well as laboratory methods!

Conclusions

Our disagreement is rooted in what is meant by universality. Russell erected a straw man, a universalist who would admit of no cultural variations in facial expression, who would therefore be demolished if agreement across cultures is less than perfect. Russell misrepresented my views, which emphasized both universals and cultural differences. Perfect agreement in how subjects use verbal labels to judge facial expressions is not predicted by my neuro-cultural theory of emotional expression, because the words used in referring to emotions are likely to be permeated by culturally variable attitudes about emotions.

The issue is whether there was significant agreement across cultures in how facial expressions of emotion were interpreted, despite all the sources of cultural variation. The evidence that Russell reviewed of 31 literate cultures is overwhelming in support of my neuro-cultural theory of facial expression. With almost no exceptions, statistically significant levels of cross-cultural agreement were obtained. Russell admitted that statistically significant findings were obtained: "I do not dispute the formal statistical finding in each study of an association between facial expression and emotion label" (Russell, 1994, p. 109).

Russell tried to undermine those findings by raising a variety of possible faults in the design of the literate culture studies. My analysis of the problems Russell raised about the subjects, the presentation order, and the facial expressions shows that none of his criticisms have any merit. Nearly all are directly answered by findings he ignored. Russell was simply trolling red herrings.

There is more substance to the questions Russell raised about the possible limitations in the use of a forced-choice response format in these studies. Perhaps, he argued, other emotions might have been recognized if only they had been included among the choices. I described new findings that show that subjects who could choose their own emotion terms to describe the emotion shown in a face evidenced as much agreement as has been reported in the many cross-cultural studies in which subjects were restricted to a forced choice. Some of Russell's concerns about the forced-choice format were also met by cross-cultural studies that used quantitative ratings on emotion scales and also obtained evidence of universality. Russell failed to fully report those findings. Instead Russell raised a possible design flaw and then failed to report a published study that did not have that possible flaw and still replicated the findings.

Russell also tried to discredit the findings from the preliterate cultures by what he called "fictional reconstructions" of what might have happened to introduce error into these studies. He was not there, of course, and each of the different investigators who did that work reported they had overcome the difficulties that they encountered. But Russell was again selective in what he reported. Russell fully reported Sorenson's account of the difficulties (which Russell elaborated on in his fictional reconstruction), but Russell failed to report Sorenson's judgment that he successfully met those difficulties. Russell also omitted our description of the steps we took to safeguard our findings from many of the problems that Russell argued might have occurred.

The preliterate culture findings are especially important because they showed that agreement in interpreting facial expressions was not limited to subjects who had a shared visual input, such as would be provided by the mass media. In the crucial data, from the most remote groups in New Guinea and West Irian, the subjects had little or no contact with the outside world. Some of these studies used freely chosen labels, others had their subjects pick the expression that fit a story that described an emotion antecedent (e.g., "he is about to fight"). And, except when subjects would not cooperate with the task, statistically significant agreement was reported.

Russell criticized the studies that used an emotion antecedent, because for him it is only emotion labels that are of import. Russell ignored the evidence (Boucher & Brandt, 1981; Scherer et al., 1986) of common emotion antecedents across Western and non-Western cultures. For me it is just as relevant to the universality thesis to associate a facial expression with an antecedent event as it is to associate an expression with an emotion label. From my theoretical perspective, emotions are not reducible to labels. An emotion label is a shorthand that stands for a number of processes and responses that occur during an emotion—for example, the appraisal of an antecedent event, physiological changes, expressive behaviors, relevant memories and expectations, and coping.

This last point implies an answer to why Russell went to such lengths trying to discredit the universality findings. Why did he not accept statistically significant findings? Why did he indulge in fictional reconstructions? Russell is interested in emotion words, not in emotion per se and certainly not in facial expression. I have not been primarily interested in emotion words but in facial expression and more generally in emotion. Emotion words were but one tool we used to understand expression, in particular to counter the position that reigned before our work that facial expression is socially learned

and completely culture specific. (Russell's historical sketch wrongly claimed that view was not prevalent; see the Appendix).

Given Russell's interest in the words themselves, it is understandable that he would focus on the fact that agreement in the use of words was not perfect, whereas I have focused on the fact that agreement was far better than chance. Both views are true. The statistically significant agreement supports the universality thesis as I have described it. The variations from culture to culture, which are considerably smaller, may be due either to error or to systematic factors associated with the language of emotion. Research is needed to clarify this.

In commenting on what I have written, Lazarus (personal communication, August 20, 1993) said such disputes are never completely settled by the data, but are resolved over time by how generative each position is—what new questions and new findings each has led to. Space does not allow me to do more than mention a few of the studies that have made use of the universality findings. When Gottman (personal communication, August 1993) considered just his measures of specific emotions shown in the face during marital interaction (he had a great deal of other nonfacial data as well), he found that the wife's display of the expression universally associated with disgust predicted how many months the couple would be separated from one another in the next 4 years. The correlation was .51, $p <$ .001.

In their classic studies of social referencing, Campos and Stenberg (1981) found that if the mother showed a universal fear expression, the infant avoided the situation (crossing the visual cliff or playing with a toy). If the mother showed a happy expression, the infant usually crossed the cliff and played with the toy.

Krause and his associates (Krause & Lutolf, 1988; Krause, Steimer, Sanger-Alt, & Wagner, 1989; Sanger-Alt, Steimer, Wagner, & Krause, 1989) reported many findings in which measures of specific universal facial expressions distinguished whether a normal individual was conversing with another normal individual or a schizophrenic patient (in remission). They also showed that one person's attribution of emotion is related to the facial expression that is shown by the other interactant. For example, anger expressions correlated −.58 with attributions by the other interactant that the person showing the anger was feeling fear (Krause, personal communication, August 10, 1993).

Ekman, Levenson, and Friesen (1983) discovered a new role for expression: as an instigator of changes in emotion physiology. When subjects followed muscle-by-muscle instructions to make the facial expressions universally associated with emotion, different patterns of autonomic nervous system activity occurred. These findings have been replicated a number of times with young and old American subjects (Levenson, Carstensen, Friesen, & Ekman, 1991; Levenson, Ekman, & Friesen, 1990) and also with subjects in a matrilineal, Muslim culture in Indonesia (Levenson, Ekman, Heider, & Friesen, 1992). Ekman and Davidson also found that different patterns of central nervous system activity accompany different emotional expressions, both when emotions occur spontaneously (Davidson, Ekman, Saron, Senulis, & Friesen, 1990; Ekman, Davidson, & Friesen, 1990) and when an expression is deliberately made (Ekman & Davidson, 1993).

Etcoff and Magee (1992), working with the universal facial expressions, used a computer to generate a series of faces that differed by a constant physical amount, running from one emotion to another. Subjects were asked to discriminate pairs of faces, categorizing the emotion shown in each. Faces within an emotion category were discriminated more poorly than faces in different categories that differed by an equal physical amount, showing that emotional expressions are perceived categorically. Etcoff and Magee interpreted their findings as contradicting Russell:

> The discovery that expressions are perceived categorically . . . refutes suggestions. . . that facial expressions do not provide enough information to define category distinctions, that people do not naturally perceive expressions as falling into categories unless forced to by multiple-choice experimental procedures, and that in perceiving expressions we map continuous dimensions of facial configurations onto continuous aspects of facial configurations. (Etcoff & Magee, 1992, pp. 237–237)

In this concluding section, I first summarized the evidence, which is very strong, and Russell's criticisms of that evidence, which I showed to be very weak. I then turned to the question of whether the evidence of an association between specific facial expressions and specific emotions has served to generate new findings and new questions for research. Let me close by considering a matter of epistemology. Russell states the following:

> If universality is a fact, then the implications are far-reaching indeed. D. E. Brown (1991) and Buss (1992) cited the existence of universal facial expressions as one of six key cases in their argument for bringing back the concept of human nature. For some theorists, universality is deeply revealing about the nature of emotion" (Russell, 1994, p. 102).

I could not agree more. For those who view emotion as a biosocial (neuro-cultural) phenomenon, the findings of some universals, as well as cultural differences, is of great import. Those who consider emotion as strictly socially learned, with little or no contribution from our biology, those who consider only nurture and reject nature, will probably always find some way to dismiss the evidence for universals no matter how strong or robust that evidence is.

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(Appendix follows on next page)

Appendix
Russell's "Partial History" Is Biased

The reader of Russell's sketch would not know that at the time that Izard and I began our cross-cultural studies of expression, the reigning belief in most of the social sciences was that facial expressions were, to use the current lingo, totally socially constructed and culturally variable. (That was indeed my own position when I started my research on facial expression. My view was changed by my findings.) Russell implied not only that Izard and I were poor scholars but also that we did not contribute very much to the research or theory on facial expression. To create such an impression, Russell's account misrepresented who did what, selectively reporting and quoting many of those he cited. Because of space limitations, I refute only his most glaring errors.

Russell quoted from one of my reviews of the literature in which I identified those who took extreme views on facial expression. I cited those who had argued that facial expressions of emotion are totally universal and those who argued that facial expressions are completely culture specific. I disagreed with both of these extremes, emphasizing in my neuro-cultural theory both universals and cultural differences. Russell claimed that a number of others had already taken such a middle view; his list included Bruner, Klineberg, Lambert, Osgood, Schlosberg, Tagiuri, Triandis, and Woodworth.

Russell wrongly characterized what these distinguished scholars did. Osgood (1966) did but one study of facial expression, and it did not compare subjects across cultures. Although Triandis and Lambert (1958) compared cultural groups, they also did but one study, and in my discussion (Ekman, 1973) of their findings, I criticized their study for having shown to their subjects the expressions of only one person. Bruner and Tagiuri (1954) reviewed the literature on the perception of people, and although they did consider facial expression in part of that review, they did not treat the issue of universality extensively. Russell singled out for special attention the contribution of Woodworth and Schlosberg (1954) and Klineberg (1940), but he distorted and misrepresented their positions and the work they did.

Woodworth and Schlosberg (1954) did not do cross-cultural studies of facial expression, although they did consider the issue in their undergraduate textbook. Their concern in their research on facial expression was how to maximize agreement in how subjects used words to describe the emotions shown in facial expressions, not universality. Woodworth and Schlosberg closed their *textbook's* chapter on expressive movements as follows:

> One emerges from a study of this topic with the conviction that there *are* certain basic emotional patterns in man, but that different elements of these patterns are selected and stressed by specific cultures. This is especially true when facial expressions serve as a conventional means of communication. (Woodworth & Schlosberg, 1954, p. 132)

It is hard to say exactly what position Woodworth and Schlosberg took. It is more a vague than a middle position on the universality of facial expressions, because it is hard to be certain whether it is only the particular facial movements that appear universally or the combining of these elemental movements into emotional expressions.

Although Russell correctly recognized that Klineberg (1940) had extensively addressed the issue of universality in his textbook, Russell was mistaken in challenging my interpretation that Klineberg did not take a universals position. Although it is true that in the early pages of his chapter on emotion, Klineberg admitted some universals, these were in gross motor activities such as crying, laughing, and trembling. Klineberg did not propose that facial expressions for anger, fear, sadness, happiness, and so on were universal.[A1] Russell omitted what Klineberg said was the most pertinent data on universality and the conclusion Klineberg drew from it. This information was provided in the five pages (Ekman, 1973) that I spent discussing Klineberg in my review of the literature on facial expression; the quotes from Klineberg that I give below also appeared there.

I quoted Klineberg:

> If expression is largely biological and innately determined, we should expect considerable similarity between these two closely related species [chimpanzee and human]. If on the other hand culture is largely responsible for expression we should expect marked differences since the anthropoids are presumably exposed to a culture only of the most rudimentary sort. [Klineberg then described Foley's (1935) dissertation, which seemed to show that humans could not accurately interpret a chimp's expressions] . . . the great difficulty experienced by untrained human observers in recognizing the emotions of chimpanzees from their facial expressions strengthens the hypothesis of cultural or social determination of the expressions of emotions in man. Emotional expression is analogous to language in that it functions as a means of communication, and that it must be learned, at least in part. (Klineberg, 1940, pp. 179 & 200)[A2]

That is not a middle position on the universality of facial expressions, but a culture-specific one. Incidentally, that is how Woodworth and Schlosberg (1954), in their review of the literature, characterized Klineberg: "Klineberg (1940) has emphasized the fact there are striking differences in the significance of facial expressions from culture to culture" (Woodworth & Schlosberg, 1954, p. 132).

Russell credited research by Landis (1924) and by Sherman (1927) as evidence that "the 1920s were a low point in the fortunes of the universality thesis" (Russell, 1994, p. 104). Russell never told the reader that Woodworth and Schlosberg (1954) criticized Landis's (1924) study. I quote from them:

> It must be admitted, however, that the range of emotions actually aroused was rather narrow, that the Os [subjects] were probably not expressing their emotions freely, and that the analytical data are difficult to handle by any recognized statistical procedure. A poker-faced individual can undergo a variety of experiences without revealing his emotions, and still the naive face may be very expressive. Thus, Coleman (1949) took movies of 12 Os, in situations roughly like those of Landis and found that the best two Os yielded pictures that were judged quite extensively. (Woodworth & Schlosberg, 1954, p. 120)

We (Ekman et al., 1972) spent eight pages analyzing the flaws in the Landis (1924) study and how Coleman (1949) attempted to correct them.

Woodworth and Schlosberg (1954) also criticized Sherman's (1927) study: "But we do not know whether this means that the infants were too young to have differentiated emotions or simply had not had time to learn expressions from adults" (Woodworth & Schlosberg, 1954, p. 130). Ekman, Friesen, and Ellsworth (1972) spent six pages analyzing the flaws in Sherman's study. Russell might disagree with both Woodworth and Schlosberg's criticisms and with Ekman et al.'s criticisms of Landis and Sherman, but it is biased scholarship to cite those studies and not let the reader know the criticisms that exist in the literature.

From how Russell wrote about display rules, some readers might mistakenly think that in coining the phrase *display rules*, we (Ekman & Friesen, 1969) had not acknowledged Klineberg's earlier contribution, nor added much to it. I quote from Russell: "Culture might determine whether natural expressions are permitted, inhibited, or exaggerated. To illustrate, Klineberg

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[A1] I thank Mark Frank for pointing this out.

[A2] In that review of the literature, I reported a reanalysis of Foley's (1935) data in the light of current knowledge of chimpanzee facial expressions, which showed that the college students were able to accurately interpret the chimp's expressions. If Klineberg (1940) had known that, presumably he would have changed his view.

(1940) cited large variation in weeping from grief. This normative control later came to be called a *display rule*" (Russell, 1994, p. 105). I acknowledged Klineberg's contribution on the control of expression as follows: "Thus Klineberg assumed that facial expressions of emotion can be managed and controlled, and that cultures have what we have since called display rules about what one should show on one's face" (Ekman, 1973, p. 177). We broadened and elaborated on how such control might occur. We specified four types of display rules: deintensify (Klineberg's main focus), overintensify, look affectless, or simulate another emotion. We discussed what factors must be considered in specifying when a display rule will be shown—including the characteristics of the expresser, others present, social roles, and social context. We distinguished between cultural and personal (idiosyncratic) display rules. And we reported the first experiment that attempted to manipulate the presence or absence of display rules (Ekman, 1972).

This is not the only instance in which Russell implied that neither our work nor Izard's really said anything new. Even if we had simply repeated what our predecessors had said—which is far from the case—Friesen, I, and Izard did do cross-cultural research showing faces to people in different cultures. Bruner, Klineberg, Osgood, Schlosberg, Tagiuri, and Woodworth did not. Each of these distinguished scientists made important contributions, both theoretical and empirical, for which they are rightly famous, but it was not for their work on the universality of facial expressions.

It is rare, of course, that any idea is completely original. Scientists build on the work of others, acknowledging the contributions of their predecessors, but that does not diminish what further contributions they make. Russell also wrongfully diminished the originality of Darwin's contribution on facial expression and criticized me and Izard for being too generous in crediting Darwin. I do not refute this point because Izard does so in his reply.

In closing, let me note that my argument with Russell's scholarship is not that we disagree, for scholars often disagree about how they interpret the development of ideas and the history of research. But this is not a matter of honest disagreement. Russell's reporting is biased. He omitted what did not fit his agenda. And it is hard not to believe that Russell did not attempt to cast aspersions on my own scholarship[A3] and contribution and also on Izard's.

[A3] Russell quotes at length (Russell, 1994, p. 131), from our book, *Unmasking the Face* (Ekman & Friesen, 1975), to show how we failed to qualify our findings in regard to limitations in ecological validity. Because we wrote this book for the layman, it does not have all the qualifications, caveats, and hedges that are typical in scientific publications. To make matters worse, Russell omitted, from his lengthy quote, the sections in which we did introduce complications, such as blends, which make the interpretation of expression more difficult. I asked Russell to replace this quote with a quote from one of my articles or books written for the scientific community. Russell first said he would drop that quote if I could furnish him with another from a scientific publication, but when I did he refused to drop the quote and instead added the other quote as a footnote. It is hard not to believe that Russell deliberately sought to create a false impression about my views by using this quote and by omitting the full text of what I wrote from the pages he quoted. This is just one of many instances in which it seems that Russell went beyond straightforward disagreement in an attempt to discredit my scholarship.

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Clarification of Publishing History

Cathy Spatz Widom, PhD, has requested that APA alert researchers and readers that her article,

> Widom, C. S. (1989). Does violence beget violence? A critical examination of the literature. *Psychological Bulletin, 106*, 3–28,

has been amplified and reprinted in a book chapter,

> Widom, C. S. (1989). The intergenerational transmission of violence. In N. A. Weinger & M. E. Wolfgang (Eds.), *Pathways to criminal violence*. Newbury Park, CA: Sage.

Through an oversight, cross-reference between the two publications and information on the copyright for this article were omitted from the Sage book. The copyright is held by the American Psychological Association.