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Klaus R. Scherer

Ways to study the nature and frequency of our daily emotions: reply to the commentaries on “Emotions in everyday life”

My co-authors and I are impressed by the wide range of issues addressed in the commentaries on our article about everyday emotions, as well as by the incisive analyses, interesting examples and rich data sets presented by the commentators. We welcome the opportunity to reply to these commentaries and thereby inaugurate an interdisciplinary debate on the nature of emotional experience, a topic that has fascinated scholars in the humanities as well as the social and behavioral sciences since antiquity. As the commentaries beautifully illustrate, in spite of the venerable history of scholarship on the phenomenon, there is still no consensus on how to define emotion or how to study it in an appropriate fashion. In reviewing the issues raised, I will highlight the major questions asked in our research.¹ What is an emotion? How many different emotions are there? How often do these emotions occur? What factors make the occurrence of a particular emotion more likely? What are the modal antecedent and response profiles for the different emotions?

What is an emotion?

There will never be a definitive answer to William James's classic question. Both scientists and laymen diverge on how to define the concept "emotion" and what types of phenomena and experiences to subsume under this category. In several of the commentaries this is an explicit or implicit issue. Thus Peter Goldie suggests that our research methodology, specifically the central question about an eliciting *event*, may have limited the type of affective states likely to be reported by neglecting emotions elicited by thinking of past or future events, or being exposed to virtual events such as in fictional material or imagination. He would be rightly disappointed if in replying I chose either of the two solutions he outlined (quasi-emotions or a special class of non-event-based emotions). My answer is a bit more complex, and it requires a somewhat lengthy explanation.

I have defined emotion as "an episode of interrelated, synchronized changes in the states of all or most of five organismic subsystems in response to the evaluation of an external or internal stimulus event as relevant to major concerns of the organism" (Scherer, 2001a: 93) and I have suggested a design-feature approach to differentiate different affective phenomena in a principled manner (Scherer, 2000, in press). I do not entertain any hope that this definition will have more of a chance of being widely adopted than previous attempts. There is a general tendency to use the label "emotion" indiscriminately, failing to distinguish "true" emotions from preferences, attitudes, moods, interpersonal stances, feelings and many other things. However, there is value in distinguishing event-based emotion episodes, characterized by a synchronization of several subsystems of the organism (and thus being relatively intense), from other types of affective states or enduring dispositions (see also Ben-Ze'ev, 2000, for a similar classification) because they correspond most to the evolutionary functions of emotion, i.e., adaptive response preparation (Ekman, 1992; Frijda, 1986; Scherer, 1984). As the definition given above explicitly states, emotion-eliciting events can be either external (e.g. somebody hits me) or internal (e.g. I suddenly remember that I missed an important appointment). Both external and internal events can also happen gradually, for example the slow escalation of antagonism in a negotiation or mulling over the implications of a colleague's utterance and becoming increasingly convinced that the person has meant to

insult me. Thus events can have a sudden onset and brief duration (like receiving an unexpected present) or they can have a gradual onset and evolve over time (like the illness of a loved person). In his comment Brian Parkinson provides a nice example of the latter (a student getting increasingly mad at a flatmate). In his analysis this is a case of an emotion that “unfolds as a dynamic self-organizing response to a diversity of changing circumstances”. Alternatively one could argue that the example can be explained by a cumulation of different effects – a bad mood at the outset, a stressful and noisy environment (“loads of people watching TV in the room”) while trying to concentrate, and the sudden realization (an “eliciting event”?), that the flatmate speaks in an unbearably loud voice (it might well be that the bad mood had been initially caused by the flatmate who might have invited the crowd). Whatever the correct explanation, it is certainly true that there are many situations in which emotions unfold because of changing circumstances (giving rise to changing appraisals), cumulating and escalating. Parkinson may well be right that such “unfolding emotions” are under-represented in our data as they may be perceived as less prototypical than sharply delimited events. But then the former might also be less frequent than the latter, as the probability of several minor factors converging, fostering a strong emotion, might be fairly low.

Much of the difficulty in defining and studying emotion is due to the extraordinary fluidity and changeability of the emotion process, which is often hard to segment into discrete chunks and even harder to describe with discrete language labels (which denote steady states and ideal categories). The nature of the emotion will change as events, and in consequence the appraisal of their implications, change. In their comment Wilhelm et al. correctly point out that events are complex dynamic processes, and emotion blends can be due to the presence of different facets of an event that have different implications and elicit different appraisals. However, in some cases a flow of events can and must be segmented into different events. For example, Wilhelm et al. refer to our lost-luggage study (Scherer and Ceschi, 1997) and argue that reporting one’s luggage lost and being informed about its retrieval will generate different emotions. We would consider these as separate events. We studied only the emotional reaction to the event “luggage not found on the belt and reporting the loss to an airline agent”. As we showed in that study, even this highly circumscribed event (from staring at the empty delivery belt to leaving the baggage-claim office) elicited very many

different emotions, almost all of them blends, as a consequence of differential appraisal of the importance and implications of the loss. However, the distinction between different events and different facets (or meanings) of the same event is not always easy to draw, as we showed in an earlier article on the dynamic unfolding of events and the changes in emotion blends reported in a telephone survey of everyday emotions in the San Francisco Bay Area (Scherer and Tannenbaum, 1986).

Emotion-eliciting events consist of many different types: they can happen on the spot (in real or virtual form), be registered by the individual's sensorium and appraised on-line; or they can be recalled from memory or fabricated by imagination and reappraised at will. Peter Goldie's elegant analysis of the "life of the mind" provides examples for such internal "eliciting events" (for events they are, having a beginning and an end, as well as occurring in sequence). His emphasis on the generation of emotion through thought processes is very germane to the appraisal tradition, which claims that it is the subjective interpretation of, i.e. the thinking about, facts and events that determines emotional responses (Ellsworth and Scherer, 2003; Roseman and Smith, 2001). In consequence the extreme richness and complexity of human thought processes are likely to generate an equally complex emotional life. There are two caveats, however. The first is that emotion-eliciting information processing occurs at different levels of processing (Leventhal and Scherer, 1987; Van Reekum and Scherer, 1997), much of it rudimentary and unconscious, and it is thus problematical to concentrate only on conceptual "thought". The second is that events and facts must be pertinent to an individual's needs, goals or values to elicit emotion (Frijda, 1986; Scherer, 2001a).

A central tenet of appraisal theory is that the very same event may be considered as highly relevant by one individual and as totally irrelevant by another, depending on the respective needs, goals or values (and coping potential) at that point in time. Thus the same event can provoke strong anger in one person, strong fear in another and no emotion whatsoever in a third. I believe that the same is true for the "life of the mind". Imagining a situation of potential danger may elicit fear or worry in one person but not in another (depending on the appraisal of probability, coping potential, etc.). Reading and pondering about the Chechen chief Hadji Murat's fate may elicit sadness in Peter Goldie and quite a different emotion in a patriotic Russian. Or it may elicit admiration at Tolstoy's skill as a writer; or

produce boredom; or no particular reaction at all. The extent to which exposure to fictional content in novels or films or listening to music, both in real life and in emotion-induction procedures in the psychological laboratory, generates emotions is an interesting issue. One can show that mechanisms other than appraisal (e.g. contagion, motor mimicry, memory recall or empathy) may be involved in the production of affective states through media exposure or music (Scherer, 1998; Scherer and Zentner, 2001). The nature of these affective responses remains relatively unexplored.

Notwithstanding these open questions, I agree with Goldie's (2000) position, as part of established philosophical tradition, that emotions have objects, they are "about" something, be it a fact or a belief. And I suggest that people in general are also convinced that their emotions are about something, are caused by what they at least believe to be a fact, and that the onset of the emotion coincides with an event (in the multiple meanings of this term explored above). This is why we asked our respondents to "recall the day of yesterday and describe, in as much detail as possible, an event that caused you to experience an emotion". I am not convinced that this formulation prevented our respondents thinking of a media event (or recall from memory) as an elicitor, if indeed exposure to media content or a recalled experience did produce a strong and memorable emotion yesterday. In a survey of students in nine European countries whom we asked to recall recent instances of having experienced a number of major emotions, over 20 percent reported a media event as the cause of their reaction (Scherer et al., 1983). However, it may well be possible that laymen have a tendency to associate the term "event" with more dramatic external happenings than the intricate internal thought processes described by Goldie and that we did indeed undersample such emotional experiences.

On the other hand, while he thinks he did, it is not certain that Peter Goldie experienced *real* shame when recalling a question that, to him, seemed silly and provoked shame in a seminar setting two weeks previously. Did he blush and turn his head away at the time of recall (Keltner and Buswell, 1997)? Did his subsystems get synchronized as required by the definition I offered above? Peter Goldie thought he re-experienced shame when the recall occurred, but was it really the same emotion or a cold cognitive copy of the original? We will never know. Clearly in a survey like the one discussed here, the criterion is whether the individual concerned claims that there was an emotional response or not. This may depend on

perceived intensity. The more resources the organism needs to mobilize and coordinate to prepare a response to a significant event, the more conscious the individual may be of the experience, and the more intensely it may be felt. All of this is quite speculative, and there is little research on intensity (but see Edwards, 1998; Sonnemans and Frijda, 1994) and absolutely none on subsystem synchronization and its link to intensity. It will be difficult and probably quite arbitrary to define the threshold of synchronization that is supposed to characterize an emotional episode in the sense of the definition suggested above. Yet if we want to distinguish emotions as evolutionarily continuous mechanisms for relevance detection and response preparation, in other words as emergency mechanisms, and thus distinguish them from minor variations of mood or well-being, we do need a set of criteria to define the different categories (which may well have rather fuzzy boundaries).

Because emotional intensity cannot be calibrated or anchored in surveys, it is not possible to ask respondents to report only experiences above a certain intensity level. This is why we explicitly asked them to report an emotional experience *even if it was weak*. We wanted thereby to avoid limiting the reported events to exceptional cases. Yet we wanted to restrict responses to those emotional experiences that had actually entered long-term memory (allowing recall within a 24–28 hour period), the assumption being that the passage of an experience into long-term storage is an indicator for its relative importance to the organism. As described in the commentary by Wilhelm et al., the passage of content from short-term to long-term memory invariably involves modifications, often in the sense of divergence from reality. But just as appraisal theorists are more interested in the subjective interpretation of an event and its consequences than in its objective characteristics (because it is the former that determines the emotional response), biased memories might be of greater value to study in an attempt to understand the mechanisms of emotion than the veridical recall of what really happened. It is most probable that what the person remembers is closer to the way he or she appraised the significance of the event. This is particularly true in the light of an important aspect of emotional experience highlighted in Averill's comment: "A person's ongoing experience is formed and given meaning by the conceptual categories or schemas that are brought to the situation." Most likely the memory trace of an experience reflects the end result of this process, including the improvisational and creative aspects Averill

mentions, as well as individual and cultural differences with respect to the frames of reference that form the backdrop of emotional meaning.

In choosing the formulation of the central question in our survey, in line with a theoretically motivated definition and by limiting the recall period to the previous day, we attempted to ensure at least some degree of comparability of the responses. Obviously respondents may well have reported a different class of experience, and one that some emotion researchers, including some of the commentators, would also consider as "emotion", had we asked "Please report all emotions you had yesterday." While to my knowledge there is no survey on what kinds of states people in general will call "emotion", van Goozen and Frijda (1993) have shown that students in different countries consider a large variety of states of mind as "emotions". Parkinson suggests that what respondents may have qualified as an "emotion" worthy of report might depend on culturally determined narratives or popular conceptions about a bundle of typical emotion features. We agree that the type of experiences searched for in the memory of yesterday may have varied widely across individuals and the two subcultures studied. Also, as Ben-Ze'ev and Revhon as well as Wilhelm et al. point out in their comment, the respondents in our study mention a number of labels that refer to moods or affect dispositions rather than emotion episodes. Still, as the recorded situation descriptions show, the responses are generally linked to an identifiable cause or event and exceed a certain threshold of intensity.

This focus on event-accountable, relatively intense emotions distinguishes an event-sampling approach from time-sampling studies. Time-sampling is appropriate for monitoring moderate phenomena that are continuous, such as temperature; it is less useful for the study of short, rare and intense events, such as lightning. In the affective domain, beeping a person several times a day is well suited for studying the variation of mood or of states of well-being. As shown in the literature, most students and many other people are perfectly able to rate their state on both a valence and an arousal dimension whenever prompted by an electronic device. The burgeoning literature on time-sampling of affect shows that this yields interesting and useful data. Respondents in time-sampling studies will also readily use discrete emotion labels from a list provided by the researcher to describe their states. Wilhelm et al. review very interesting data from their own computer-assisted diary studies in

Swiss families as well as from other research groups that amply demonstrate this. But one has to be aware of potentially serious response biases in such time-sampling studies. As these commentators mention themselves, the sound of the beeper is a strong demand characteristic in itself. There is pressure to report something likely to interest the researcher or to demonstrate that one does not lead a dull life. There are credible reports that respondents have made up emotional events or asked friends around them to help out with a juicy episode. In any case the participants in such studies are primed to report emotions, and it is not unlikely that they experience and label the world and their own feelings differently while participating in a diary study. Undersampling intense emotions as they occur may happen because respondents may not be in the mood to respond to the beeper in highly affectively charged situations or be able or willing to describe the situation and their feelings.

Yet in many cases reports of emotions in diary studies are likely to be veridical. One sits in a boring lecture when the beeper sounds, and one reports boredom. One listens to the latest hit on one's MP3 player in the bus and one reports elation. One again has trouble with Windows on one's portable and one reports anger. All of these are event-based experiences that might well fit the bill of the definition suggested above, occurring frequently enough to be reliably picked up in time-sampling studies. However, the probability that one will capture *intense* emotional experiences is probably rather low because of their rare occurrence and because of some of the reporting and self-presentation biases mentioned above (respondents probably rarely feel anonymous). Wilhelm et al. report that, for anxiety, anger and sadness, more than half of the respondents indicated that they experienced the respective emotion only "a bit". In our survey study, of those respondents who reported the same set of three emotions, only 8 percent described the intensity as "weak" (which is probably comparable to "a bit"), whereas 62 percent described it as "strong" or "very strong", the remainder using the label "medium". There is a serious question whether the emotions *time*-sampled respondents report to have felt "a bit" are comparable with those *event*-sampled respondents describe as strong or very strong, even though the same label was applied. One could argue that a person able to experience a *strong emotion* every 10–20 minutes (see report of a study by Myrtek et al. in the commentary by Wilhelm et al.) qualifies as an emotional athlete. And while I can conceive that 90 percent of the population may

experience at least one emotion “a bit” every day, I am not convinced that *everyone* has a strong emotion, one which will remain in memory over a lengthy period of time, *every single day*. I concede of course that it does happen that one experiences several strong emotions in a single day and that, as several commentators have noted, our research procedure does not allow respondents to report more than one. This will lead to an *underestimation* of the probability of everyday emotion experiences. On the other hand, as some commentators mentioned, some respondents may have waited until they had an episode to report before they returned the questionnaire, which would lead to an *overestimation*. These biases might cancel each other out.

How many different emotions are there?

This question cannot be satisfactorily answered as long as there is no consensual agreement on a definition of emotion. I have suggested, on the basis of theory (Scherer, 1984, 2001a), that there are as many emotions as different appraisal combinations. Judging from his comment, Averill would go beyond this claim by adding the variation that ensues from contextual interpretation and rhetorical use. Ben-Ze’ev and Revhon also highlight the complexity, and the general tenor of the commentaries by Katz and Le Breton suggests that they, too, endorse the existence of an enormous diversity of emotional experiences, as determined by features of the individual, the culture and the specific situation. We have used the data from the Swiss survey to argue for the viability of such a complexity/diversity view, interpreting the evidence as incompatible with attempts to reduce the richness of the human emotion repertoire to a small number of basic emotions, let alone the position of a feeling state in a two-dimensional valence-arousal space. Contrary to Ben-Ze’ev and Revhon’s assumption, we have not used the reported frequency of different emotions to make this claim. I agree with them that the conceptual organization of the emotion domain cannot be derived from a frequency distribution, and that typicality and impact do not depend on high frequency. In fact, as to impact, I would expect the opposite: high-impact emotions should be rare since they are much more costly for the organism. This is supported by the finding reported in the commentary by Izard et al., that the most infrequently reported emotions are the most dreaded ones.

Instead we based our argument on the large number of verbal labels describing emotional states that were spontaneously produced by the respondents when we asked for a free description of the experience. As any use of a verbal label already involves categorization, the underlying feeling states must be even more variable than the number of labels used. I see this as evidence that the primitive organization of feeling consists of highly specific and complexly organized *qualia*, which can be seen to represent the integration of the component processes during an emotion episode (see Scherer, 2004). This directly contradicts Russell's (2003) claim that core affect, i.e. the primitive organization of feeling, consists of a valence–arousal combination that is then further differentiated in the process of labeling. I have argued (Scherer, in press) that the process works the other way around: highly differentiated *qualia* can be, and often are, projected into a valence–arousal space when the respondent is required to do so by a researcher or when at a loss to find an appropriate label (which I expect was the case for those respondents in our sample who reported positive or negative feelings rather than a specific affect label). In this sense I agree with Ben-Ze'ev and Revhon that it is useful to maintain a positive–negative affect dimension.

In consequence we interpreted the richness of the reported emotion repertoire as an argument against a strong version of a basic emotion theory (postulating specific patterns of evolved response organizations in the form of neuromotor programs; Tomkins, 1962). I should note here that there are few theorists who would take such a strong position today. Ekman (1998) has suggested that emotional responses are organized by “open programs” that can be modified by experience and he has proposed a neuro-cultural theory of emotional expression that combines biological, psychological and cultural components. Similarly Izard has defined emotion as a bio-psycho-social phenomenon and he stresses, in the commentary written with his collaborators, that, while there may be disagreement on details, there seems to be wide agreement on the determinants of emotion and their interactions. In the commentary by Ben-Ze'ev and Revhon, the defining features of basic emotions are enumerated, but it is difficult to find a clear set of predictions in the literature (Ben-Ze'ev and Revhon's mention of changing functional requirements in the course of evolution is a very important consideration that merits further development). However, the very notion that some emotions are more fundamental

or basic than others has strongly affected theory and research, and there is a tendency to limit research to what is sometimes called the "Big Six" (happiness, sadness, anger, fear, disgust and a variable sixth element). On the basis of our data we have argued against this widespread tendency to underestimate the diversity and complexity of the emotions.

As noted by Averill, our methodological procedure of first reducing the 775 labels to 38 categories and then using classification algorithms to combine some of these to a happiness and an anger group seems to contradict our plea for diversity. While the notion of *modal emotion families* (see Scherer, 1994) avoids the a priori assumption of evolutionary stable programs, it still masks the uniqueness of the individual members of the families. I agree with Averill that in many cases one can learn more by highlighting the differences between family members than by examining their similarity. Thus Banse and Scherer (1996) have strongly argued for the need to distinguish between hot and cold anger (and other such pairs) in order to understand their differential vocal expression signatures and allow replication of empirical findings. In the case of the Swiss survey we decided to use larger categories for a very pragmatic reason: in spite of our large sample size, many cross-tabular analyses would have become impossible because of low cell sizes. Obviously this carries a heavy price in loss of detail and precision, as amply documented in the commentaries by Katz and Le Breton, who miss the kind of "thick description" they prefer. However, I do feel that, just as individuals can project their complexly differentiated feeling state into a valence-arousal space, so too can they classify it into a hierarchy of higher-order emotion families that share a number of features (see also Shaver et al., 1987). The study of such classification processes and the nature of the underlying similarity structures, especially across languages and cultures, remains an important task for the future.

How often do these emotions occur?

As argued in our article and as nicely seconded in the commentaries by Averill and Ben-Ze'ev and Revhon, the relative frequency of different emotions in everyday life is important for our understanding of emotion mechanisms as well as for actuarial purposes.

I have already discussed the idea that the frequency of reporting the experience of an emotion may strongly depend on its intensity. Many other factors will also play a role, and the commentators have not failed to point these out. We are of course aware that the precision with which we report the relative frequency (and thus the estimated probability of occurrence) of emotions during any given day in a given population (with one digit after the decimal point) is quite illusory.² We are also aware of the large margin of error for our estimation of the percentage of the population experiencing an emotion on a given day, as demonstrated by the detailed analysis in the commentary by Wilhelm et al. and my discussion above. However, we suggest that the rank ordering of the frequency for the different emotions is a fairly reliable result, especially as it fits the pattern of data obtained with a different methodology in our large-scale study of emotion antecedents and reactions with students in 37 countries (Scherer, 1997; Scherer and Wallbott, 1994).

In addition the data from the Fribourg family study reported by Wilhelm et al. yield a very similar rank order (in terms of percent of respondents mentioning the respective emotion; see their Table 1 and our Table 2 in the target article), even though obtained with a very different method – computer-assisted diary method: happiness/joy > anger > sadness, anxiety. Furthermore, with the exception of shame, our data correspond rather nicely to Izard's old but rather neglected findings on the relative frequency of experience and preference for particular emotions reported in the commentary by Izard et al. The case of shame is most interesting and illustrates why the frequency of certain emotions may throw light on important change in values and socialization patterns in the process of social change (see commentary by Izard et al.; Scherer, 2001b). While too complex to pursue in the present context, this issue deserves further attention. Izard et al.'s commentary on the relationship between the earlier finding that anger is the most preferred negative emotion and the curvilinear relationship between anger experiences and life satisfaction that we found in the Swiss survey is also worthy of further exploration. In fact the relative desirability of different emotions, at a point in time when emotional experiences seem to be highly valued (Scherer, 2001b), is an issue that is all too rarely addressed.

Parkinson and Wilhelm et al. remind us of the multiple possibilities for self-selection biases to affect our frequency data.

Interestingly, however, different biases are likely to produce rather different, often opposite, effects (see also the example on self-selection discussed above). Whereas Parkinson expects an over-representation of negative emotions (because our study was introduced as concerned with health, potentially encouraging respondents convinced they suffered from bad health to report the accompanying negative emotions), other critics have suggested that positive emotions might be over-represented, as individuals suffering from depression or other negative emotions might not have the motivation or energy to fill out the questionnaire and send it back. (Wilhelm et al. suspect that we sampled a “well-functioning” subpopulation.) While I do not want to belittle the problem of biased respondent selection, this example shows the difficulty of assuming, without further evidence, the operation of particular selection factors. Clearly there are many factors that determine who will fill out the questionnaire and return it, and one should not underestimate the role of pure chance in this process. Parkinson and Wilhelm et al. also mention a number of potential response biases, such as the mood-congruence effect, the negative retrospective recall bias and others. Again there is no denying that such factors are likely to play a role. However, it might be useful to ponder what the relative impact of these factors might be. For example, the mood-congruence effect is not an “invariable and universal phenomenon” (Forgas, 2003: 599), and when it is found, the effect size seems generally rather low. The same is true for many of these effects, which are often only barely significant or cannot be systematically replicated.³ In addition many of the response-bias or priming effects have been studied, with experimental stimulus material that is of no great relevance to the participants, in situations of rather obvious mood inductions. It is questionable whether such mechanisms will have a significant impact when there is no strong mood induction (or likelihood of a specific mood being systematically present) and the event to be recalled is important for one’s well-being and happened only yesterday. The negative retrospective recall bias, discussed in detail by Wilhelm et al., seems difficult to demonstrate for the *occurrence* of single events, as occurrence or frequency are necessarily judged in retrospect.

What factors make the occurrence of a particular emotion more likely?

A large part of our article is devoted to analyzing “risk factors” for the occurrence of emotion.⁴ Surprisingly the contextual risk factors, social relations and environmental situational factors that can affect the probability of emotion elicitation, were rarely addressed in the commentaries. Wilhelm et al. correctly point to a confusion between situation and individual identity in our study, but this should work against finding significant situation differences as strong variation between individuals will increase error. In contrast the issue of emotionality as an affective response disposition and its surprisingly strong effect on the probability of which emotion was experienced yesterday, drew more comment. I am very grateful for Izard et al.’s endorsement of these findings based on their own data and their brief review of the literature. I also think that there is strong evidence in the literature for the existence of affective traits or dispositions, and if these do exist they should produce the effects we find. Thus the pattern of the results and our interpretation are consistent with an abundance of theoretical assumptions and massive empirical evidence published in major journals. In our article we discuss the possibility of a methodological artifact due to the fact that respondents filled out both parts of the questionnaire at the same time, and we come to the conclusion that, while this cannot be ruled out, it is not very probable. However, the anonymous reviewers of an earlier submission of the article and some of the commentators (e.g. Parkinson) were not convinced.

Before addressing the core of the issue, it may be important to clarify the dependent measure we used to assess dispositional emotionality, i.e. asking respondents to report on the frequency with which they tend to experience each of 14 emotions. As we explain in the article, we used this measure exclusively to obtain estimates of trait emotionality such as trait anxiety, trait anger, etc., instead of using established psychometric scales (like the STAI or the STAXI; Spielberger et al., 1999), which would have been impossible to use in the context of the survey. Also we wanted to obtain an estimate of dispositional tendencies for emotions for which there are currently no established scales. We did not expect to obtain any reliable information as to the *real frequency* with which respondents have experienced these emotions in the past, and we said so in the article. Thus we agree with Parkinson that the reports on experi-

enced frequency are subject to many biases and would have to be considered as artifact-ridden if they were to be used as indicators of realistic occurrence measures. Where we have been misleading is in reporting the results. Whereas we do stress the use of the measures as emotionality estimates, we report some of the results (e.g. the correlations with sociocultural background factors) as if they could be interpreted in terms of actual frequency. Also in some formulations we neglect to stress that these are *reported* frequencies. What we should have done is to talk exclusively about higher or lower trait emotionality for each emotion.⁵

One “bias” that was not mentioned by the commentators is the one we hoped to capitalize on: the tendency of individuals who are high on certain emotion dispositions to report that they experience the respective emotion disproportionately more frequently. One way to test this assumption is to conduct studies that allow correlating subjective frequency reports with established scales of trait emotionality (at least for those emotions for which the latter exist), as well as with peer report, to provide construct validity data. This remains to be done.

The central issue is whether we can interpret our data as showing that individuals with a high level on a certain emotion disposition are more likely than those scoring low on this trait to have experienced the respective emotion the previous day. We agree with Parkinson (and some anonymous reviewers) that we cannot rule out the operation of a number of reporting tendencies. Specifically Parkinson mentions the availability bias (anger instances and labels being more readily available to irritable people) as well as the differential availability of specific emotion schemata more generally. However, we feel that three factors make the operation of these biases rather unlikely: (1) the respondents reported very concrete, often unusual, events with rather obvious emotion-eliciting potential; (2) the respondents used an enormous variety of emotion labels to describe the experience they had yesterday, while the disposition measure consisted of only one standard label; and (3) the disposition measures for different emotions correlate and yield well-established clusters such as depressiveness/anxiety.

Demonstrating the effect of dispositional emotionality on the actual frequency with which the individual experiences the respective emotion represents an almost unsolvable logical and empirical paradox: if emotional experience in the sense of subjective feeling can be measured only via self-report (see Scherer, 2004), it is impossible to

establish a causal link between trait emotionality and the actual probability of experiencing those emotions in real life. Both variables are necessarily based on self-report, which will automatically produce suggestions of the operation of common method variance and response tendencies (even if the measurements are separated in time). At the same time there are good theoretical arguments for assuming that individual differences in motivational structure, self-concept and appraisal biases predispose certain persons to interpret events systematically in a way that renders the occurrence of particular emotions (including emotional disorders) more likely (see commentary by Izard et al.; Kaiser and Scherer, 1998; Roseman and Kaiser, 2001; van Reekum and Scherer, 1997). Until a clever way of experimentally disentangling the causal chain and fine-tuning the measurement operations has been found, it seems to be a matter of taste (or belief) whether one sees artifact or effect in such data.

What are the modal antecedent and response profiles for the different emotions?

This issue was hardly touched upon by the commentators. It is true that self-report data on appraisal, on the one hand (in our study only represented by the causal attribution dimension), and physiological symptoms and motor expression, on the other, do not provide much detail and may not be very trustworthy. Still, some interesting patterns emerge from our data. In particular the data on differential intensity (see commentary by Ben-Ze'ev and Revhon) and duration of different emotions provide insight into generally neglected features of the subjective-experience component of emotion, especially as they confirm earlier results obtained with a different research paradigm (Scherer and Wallbott, 1994). Parkinson makes some intriguing suggestions as to how to use the data to study and evaluate *exceptions to received predictions* about emotion-specific response patterning that are well worth following up. He raises the very timely issue of whether other-blame is a necessary antecedent for anger (see Berkowitz and Harmon-Jones, 2004, and the commentaries on this article in the same issue of *Emotion*) and interprets some of our data as evidence against such a strong claim. Tanja Wrانik (2004), in a series of three experimental studies conducted in our laboratory, has also shown that self-anger is an important,

and often neglected, form of anger, which is determined in part by internal attribution (self-blaming).

After having touched upon the major questions posed in our article and the commentaries that are pertinent to these, in conclusion I briefly address three general issues raised by the reviewers: (1) representativeness and generalizability, (2) thick vs thin description and (3) need for triangulation of findings among different research paradigms and methodological approaches.

Representativeness and generalizability

Representativeness concerns generalizability. We do not claim that we can generalize to the Swiss population as a whole, as we did not obtain a random sample. We qualified our sample as quasi-representative. By that we mean that we can roughly generalize to a cross-section of the Swiss population with respect to the two major language/culture groups and a distribution of sex, age and education that is close to the national average. We agree with Parkinson and Wilhelm et al. that there are many potential self-selection mechanisms that may have operated, in fact so many divergent ones that it seems rather unlikely that we ended up with a group of hypochondriacs, of exceptionally happy and active people, or of habitual complainers. As often in large samples, the biases may cancel each other out.

Most importantly, different kinds of self-selection factors are operative in all kinds of empirical-data-gathering enterprises that recruit volunteer participants. As Wilhelm et al. mention, there can be little doubt that there are massive self-selection factors for groups of participants willing to submit their lives for weeks to a strict regime of keeping a meticulous diary or being at all times at the beck and call of a beeper in time-sampling studies. I also suspect that not everybody would be willing to submit to intensive participant observation or lengthy in-depth interviewing, as described by Jack Katz. In much of anthropological fieldwork there is not only massive self-selection of informants but also the danger of serious observer bias (see the controversy surrounding Margaret Mead's Samoan research; Freeman, 1999).

It would take an article of its own to review the selection and response biases that beset studies with college undergraduates for

course credit or payment. Surprisingly these well-documented artifacts (Rosnow and Rosenthal, 1997) are generally not even discussed in the psychological literature even though a vast majority of generalizations on psychological mechanisms (including emotion and social behavior) is based on unreplicated studies with American undergraduates (see Scherer, 1993). Our group in Geneva also runs such studies with our undergraduates, and we agree that this is a necessary part of systematic, experimental research. However, there is clearly a need to complement these studies with empirical data gathering that can pretend to a somewhat larger degree of ecological validity. Such work, which for obvious reasons does not meet all the standards of experimental control, is often critically assessed by psychological researchers. Unfortunately there is sometimes even a tendency to apply criteria in reviewing field studies that are rarely applied to laboratory research with students in psychology courses. This is the case, for example, with respect to representativeness and generalizability.

It is instructive to examine what actuarial data on everyday emotions would look like if we had conducted our survey exclusively with students. In fact we did ask a total of 227 psychology undergraduates at the University of Geneva in two waves, during the years 1997 and 2000,⁶ to fill out the same questionnaire that was used in the quasi-representative postal survey. We find highly significant differences between the student and the population samples for most variables. Virtually all students report having experienced an emotion “yesterday”. As with the beeper in time-sampling studies, there are strong demand characteristics to fill out the questionnaire “correctly” when it is distributed in class. As one might expect, the reported emotions are significantly shorter and less intense, and there is less of an effort to control them. Concerning the types of emotion, students report significantly more anxiety, fear, tension/stress and generally negative emotions (but less despair) than the population sample. With respect to positive emotions students report more (active) joy and less (quiet) happiness than the population sample. For dispositional emotionality we find massive differences (which only partly correspond to the differences in actual emotion reports, contradicting an availability-bias explanation): the (psychology) students are generally significantly lower on positive emotionality and higher on depressiveness and anxiety.

Thick vs thin description

I am very happy that David Le Breton and Jack Katz have accepted to comment on our article but I have very little to reply, other than that I agree with most of what they say (which is not surprising since I also had some training in sociology including micro-sociology). I agree that a *thick description* of emotional experiences in real-life settings, obtained while they unfold before the observer-interactant, is vastly more satisfying and informative than the *thin description* afforded by psychological experiments, including field studies, not to speak of the extremely impoverished information gathered by a survey questionnaire like the one used in our study. I also agree that it is necessary to deconstruct the cultural-meaning structures in which the emotional episodes are embedded and that the respective individuals bring to the social situations in which emotional life takes place, an activity which requires a type of analysis that cannot be limited to the counting or measuring of observable responses, including self-report. However, one needs to recognize the cost involved in such discovery methods – the subjectivity of the interpretation⁷ and the limitation to single case studies, which both constrain generalization. Also, because there is a reasonable chance of hitting upon outliers (given the self-selection mentioned above) and because participant observation or empathic interviewing will reinforce the natural tendency toward self-presentation (which, as Erving Goffman, 1959, has shown, pervades our social behavior), conclusions from such work can be quite misleading. I do not share Jack Katz's somewhat skeptical attitude (expressed at the end of his comment) with respect to scientific and methodological pluralism and the possibility of building bridges between the disciplines (and between factions within disciplines). This brings me to my last point.

Triangulation of findings from studies using different methodological approaches

I am most grateful to Brian Parkinson for highlighting the need to examine the phenomenon under study, emotion, from different angles, with different methods, and using different degrees of analytic resolution, on the one hand, and offering different types of generalizability, on the other. This is our only hope to escape

the limitations, constraints and artifacts of any one method or paradigm. Wilhelm et al. provide a beautiful example for the great potential of such triangulation by comparing the data from their impressive family-diary study to the findings reported in the target article. They do a very good job of systematically identifying the differences in methodological approach, discussing how these constrain the interpretation of the results and suggesting interesting alternatives. Importantly they also draw very constructive conclusions as to how further work can benefit from the lessons learned from the different approaches and their comparison.

I would even go a little further and suggest, in addition to data triangulation – which could and should include the kind of contextual analysis of selected bits of reality in sociocultural settings as described by both Katz and Le Breton – *conceptual triangulation*, which would draw in the kind of conceptual analysis (and often poignantly constructed or selected examples) in which philosophers like Peter Goldie or Aaron Ben-Ze'ev excel. Hopefully the rare chance that emotion researchers have been given in this issue of *Social Science Information* to discuss their widely varying approaches in response to our “audit” of Swiss emotions will be just the beginning of fruitful debate and exchange across the disciplines contributing to an emerging Affective Science.

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Notes

1. Given the exceedingly short period of time available to prepare this reply, it was impossible to involve the co-authors of the original article in the writing. In consequence I am exclusively responsible for the opinions expressed. I thank Tanja Wranik and Ursula Scherer for their suggestions.

2. Although we have been much surprised by the stability of the data for two independent samples with a four-year interval, with an event like 9/11 occurring just before the second wave.

3. There is a serious file-drawer problem in this area, with negative findings often not being published.

4. We chose this label because of our use of the odds ratio in our analyses and because of the concise meaning of the term. We realize that we expose ourselves to the risk of being seen as adopting a disease model (see Averill and Wilhelm et al., this issue). Obviously we do not adopt such a model nor do we believe that emotions are dysfunctional. We use the word risk as a shorthand for “augmented probability of occurrence”.

5. We did not correct these errors in the text because commentators would not have had a chance to change their manuscripts accordingly.

6. There was no significant difference between the results for the two years.

7. A well-known sociolinguist of hermeneutic persuasion once told me in a heated discussion that the only validity he was interested in was the validity of his own interpretation.

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