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A man touching a woman: Unpredictable outcomes

Predictable commentary on target article by Fogel, 1992

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Introduction

In the first part of his article, Fogel attempts to give a general explanation of psychological development and of behavioral changes. According to him new behaviors appearing in the course of development are emergent outcomes of dynamic relationships between partners of the systems under consideration (dyad, triade, etc). From his point of view new behaviors demonstrated by individuals cannot be explained by their history, but are only the emergent product of their actual exchanges. Out of these interactions arise '(1) the identification and regulation of those parameters whose changes affect the quality and quantity of movement, and (2) the identification of the information required to coordinate perception and action' (p. 401). Contrary to this general statement, I consider human behavior as being fundamentally the product of phylo- and ontogenetic history. From birth infants are able to identify certain pieces of information and parameters (certain critical variables) eliciting coordinated actions and consequently giving rise to the quality and quantity of infants' movements in their interactions with the social and physical environment. These capacities result from his/her phylo- and embryogenetic history and must have some substrate that psychologists usually call anticipation

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and planning capacities, long-term memory or representation (to mention only some possible denominations).

In the second part of his article, Fogel illustrates his discourse by research done during the last ten years. He shows, by means of photographs, how what he calls the attention–posture synergy or the relation between posture (allowed by the mother) and attention (of the baby) oriented away from the mother is an emergent property of the dyadic interactions during the first six months of life. Nevertheless, all the dyads studied reach the same final result ('infant facing and gazing away') but by various roads (synchrony vs. asynchrony between partners).

In the third part, Fogel suggests the types of research that could be done from his perspective: in the field of therapeutic interventions on the one hand and in the field of guidance like for example in walking behavior, on the other hand.

At the end of each part of his article, Fogel expresses statements disconcerting by their generality, such as 'communication is constituted by a dynamic interaction, rather than regulated by a set of schematic rules or simple imitations' (p. 406) or else 'the psychosocial meaning of a movement is not genetically pre-adapted, but rather depends on the temporal and spatial relationship of the movement in combination with all other features of the social system, including its cognitive and motivational aspects' (p. 417).

Fogel's project has some similarities with Piaget's attempt to explain sensorimotor development without resorting to the concept of representation (Piaget 1936/1977, 1967/1971). For Piaget, as we know, during the beginning of the sensorimotor stage, schemes have no existence outside immediate actions or interactions; they constitute biological structures *inherent to* the subject's functionning in opposition to subsequent mental schemes or structures *produced by* (or resulting from) this functioning; these new structures define for Piaget the emergence of consciousness and intentionality, as well as of psychic or mental processes. I have recently presented and criticized this position (Mounoud 1991).

Critique of the discourse

Systemic approach of mother–child relationships has a long history and it seems questionable to write a target article in such a field with

so few references to its past (no reference was made in particular to J. Bowlby, W. Kendon, L. Sander, E. Wertheim and P. Wolff, to quote only a few authors), as well as to the various origins of this approach (von Bertalanffy, G. Bateson, K. Lewin, G.H. Mead, etc). I would like to remind that Kendon (1982) has recognized Mead as the true precursor of the interactionist approach. Already in 1934, Mead wrote 'social relationships, accordingly, are not the consequences of previously developed selves's communicating; they are the emergent products of the process of interaction'. Without any of those references, I am tempted to make the same commentary as Horowitz did at the 22nd Minnesota Symposium on Child Psychology entitled 'Systems and Development', when she spoke about new jargon for old problems: 'my caution is against a new jargon to address old problems and a possible overuse of systems concepts' (Horowitz 1989: 212).

It is true, and we can concede this to Fogel, that development in general, and social development in particular are still too often exclusively described as an invariant succession of steps. But it is equally undeniable that development is charaterized just as well by 'universal' aspects ('that appear in all normal members of the species as they develop') as by 'non-universal' aspects, to quote Horowitz again (1989: 215). It is also necessary to distinguish, as I have done elsewhere, between non-specific and specific roles of the environment in relation to the structural and functional aspects of behavior (Mounoud 1979/1981, 1984, 1992). I would also like to point out that presently environment is considered as producing selection and impoverishment as well as enrichment and differentiation (Changeux 1983; Edelman 1987; Mounoud 1990).

It is also true, again to Fogel's relief, that infants are still too often considered in isolation, independently of the social system they belong to. But it is equally obvious that from the beginning of the century many researchers have fought against this tendency. As an example, I will mention Balint (1935) who criticized psychoanalytic theory as a 'one body psychology'.

It is again true that infants are integrated in a social context. But what Fogel does not take into consideration is the fact that this social context has to be charaterized by certain features or qualities in order to provide the baby with a satisfactory supportive frame, a necessary condition for his/her harmonious and well balanced development. I would like to take the opportunity to mention Winnicott (1958) and

Harlow (1959) who have proved experts in qualifying what could constitute a 'facilitating environment' at the various developmental steps in infancy as well as the recent work done by Corboz and Fivaz on the supporting framing in dyadic and triadic systems (Fivaz 1987; Corboz et al. 1989).

Finally, it is obvious that when two individuals interact – 'a man touching a woman...' (p. 417) to use Fogel's example – it is not possible from the very particularities of each partner to foresee exactly the form and content of their relationship ('unpredictable outcomes'!) which will eventually constitute in a certain sense emergent properties of the system. But in order to grasp correctly the dynamic of the interaction, it is important to define each partner's history by means of what psychologists call scripts, schemas, memories, representations producing various expectations and anticipations. These schemas or scripts can be more or less general, more or less specific. They determine sets of *potential behaviors*. Now, Fogel rejects all 'prescriptive' theories in such an extreme way that his conception is caricatural. He reaches a peak when he writes 'Furthermore, there is nothing in the dynamic perspective specifying that the relationship needs to be tutorial, asymmetrical, motivated or even positive' (p. 405). Without any of these features it is possible to assert without taking risk that no developmental process can take place. Furthermore, Fogel should agree with such a judgement since he writes without fear for contradiction that 'the psychosocial meaning of a movement... depends... on other features of the social system, including its cognitive and motivational aspects'. Similarily in the last paragraph of his article, he recognizes the asymmetrical form of the adult–child relationships, 'Forms of co-regulation in symmetrical social relationships are likely to be different than those found in asymmetrical adult–child relationships' (p. 419). Consequently, it would be necessary for Fogel to specify precisely which roles these cognitive, motivational or asymmetrical aspects play and how they work (after having denied the necessity of their specification).

Critique of the data

From a longitudinal piece of study in preparation, Fogel describes mothers followed weekly with their baby between 1 and 6 months in

the following way. Their behaviors could be described by two components:

- a first component, stable over time, drives the mothers to produce more position changes to her baby when s/he looks away than when s/he is looking at her;
- a second component, variable over time, drives the mother between 1 and 6 months to spend an increasing amount of time holding her baby upright.

Fogel strongly rejects the idea that mothers could have acquired during their onto- and phylogeneses event schemes or representations which could determine these two types of behavior. He prefers to think that despite their generality ('universality') these two types of maternal behavior spontaneously emerge as properties of the dyadic system. 'Adults' action, no less than children's, is (co-)constructed as part of a process of mutual dynamic interaction' (pp. 401). 'Neither partner has a foreknowledge or scheme of the underlying parameters and task invariants; they must be discovered via interaction' (pp. 403–404), '... a process of co-regulation in which the adult changes as much as the child' (p. 417). In order to be consistent, Fogel should have said that these parameters are discovered only during the time of the interaction and that outside this interaction they have no existence (at all), failing that he would have to admit the notion of schema. In any case, starting with the two general components of mother's behavior, Fogel describes two possible evolutions of the attention–posture synergy: one category of dyads for which infants' behavior 'gazing away' is synchronized with the postural facing away as held by the mother and another category of asynchronous dyads for which the infant's 'gazing away' appears without (i.e. a few weeks before) the 'looking away posture' realized by the mother. Despite divergent synergies all dyads studied by Fogel 'have negotiated a posture–attention relationship oriented away from the mother' (p. 415).

Again from the same longitudinal study, Fogel describes all the babies as spending an increasing amount of time between 1 and 6 months gazing away from the mother. He does not distinguish between frequency or duration of the babies' gazes as do some authors (cf. for example Friedman et al. 1976). Of course, this behavior is presented as an emergent property. Consequently, this behavior should be specific to a given type of systems (dyads). Nervertheless, as already

mentioned, this behavior seems independent of the particularities (or specificities) of the dyads under study. Fogel's refusal to consider in his conceptualization motivational variables (such as for example the attention-habituation) does not allow him to call upon various economical explanations. Once more, I will quote Horowitz (1989: 216): 'Even in the analysis of emergent universal behaviors... those aspects of the acquisition that involve learning components and the operation of contingent feedback principles must be amenable to a learning analysis'.

I would have expected at least a more articulated demonstration of Fogel's dynamic interpretation in order to understand for example how various degrees of stability in a dyadic system could be defined or also how attractors could be characterized. In the first research presented by Fogel, he actually tried to establish an invariant relationship between inclination of the infant's seat (i.e. the postural positions) at 3, 4 and 5 months of age and various percentages of infant's gazes at his/her mother. Mothers were requested to maintain 'a similar style of interaction' despite the variation of a basic component of the system. Behavior requested from the mother should have been impossible in Fogels conception since he considers that 'if one component changes, the entire pattern of action may change as the system settles into a new synergy' (p. 415). Could it be that in some cases prescriptions are actually working?

To conclude, I will examine the following statement suggested by Fogel's article: in order to produce a given result on partner B (the infant), it is necessary that the action of partner A (the mother) acts upon the relevant parameters (of the situation). With regard to such a (obvious) statement, I will briefly consider two possible situations:

(1) If these parameters are known by partner A (the mother needs to have 'some form of representing a more skilled action', p. 399), then we are in a situation of prescription or of guidance excluded by Fogel. Nevertheless, I would like to underline that guidance is efficient only if A acts taking into consideration what is expressed by B. (Whether A is aware or not of these parameters is another problem that I will not consider here.)
(2) If these parameters have to be discovered and isolated, then A could be considered in a problem-solving situation: on which parameter(s) acting (in order) to obtain the expected result or

reaction; I would at least attribute to A (the mother!) the capacity to generate hypotheses. The interaction becomes a necessary condition for the solution. Spontaneous emergence of the solution constitutes Fogel's response.

Conclusion

As concluding remarks I will apply Fogel's ideas to explain the emergence of his new theory, the *social dynamics* in a life-span perspective. The re-conceptualization (p. 402) or the new conceptualizing schemes (p. 399) proposed by Fogel should be emergent properties of the social system he belongs to. So it is not his own attempt to accommodate or to conciliate previous conceptualization or theories to new experimental data that have produced Fogel's new ideas, as would have explained the first interpretation rejected by him and called *schematic interactionism*. It is neither under the influence or the guidance of other theories or conceptualizations such as the dynamic systems approach (p. 402) that Fogel could have appropriated his ideas, as would have explained the second interpretation rejected by him and called the *sociocultural interactionism*. His new ideas only express an emergent product of his actual interactions with the members of his community (cf. bibliography for explicited or consciously recognized members, but 'representation need not be accessible in symbolic form'). Fogel's reconceptualization was not entirely predictable (p. 414); it is totally new and original since the components he used were only loosely linked before (p. 402) and none of these components carries the final macroscopic form of the target article (cf. p. 402). His reconceptualization is not supposed to drive or to prescribe his research or its experimental design: 'clarification of one's goals as well as specifying what needs to be done in the immediate future to improve the skill [Fogel's reconceptualization, my comment] emerges spontaneously through social discourse' (p. 399). It is enough to wait for the emergence of his... predictable response!

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