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Authors
Shirai, Yasuhiro
Yap, Foong-Ha

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EXCHANGE

In Defense of Connectionism

Yasuhiro Shirai
Daito Bunka University, Tokyo, Japan
Foong-Ha Yap
Tai Po Sam Yuk Secondary School, Hong Kong

Connectionism found its way into the pages of IAL in the last two issues, with Shirai (June, 1992) advocating a connectionist framework to explore a more integrative account of second language acquisition phenomena, and Fantuzzi (December, 1992) questioning the merits of the connectionist paradigm, criticizing in particular Shirai's connectionist accounts of the phenomenon of language transfer. As researchers advocating a closer look at connectionist accounts of language acquisition phenomena, we welcome Fantuzzi's discussion of the criticisms that have been leveled against connectionism, for we believe that the field of second language acquisition stands to benefit from a greater awareness of the debate surrounding the possibilities and limitations of connectionist conceptualizations and formalizations. However, we believe that Fantuzzi's criticism of Shirai (1992) was misguided, in large part because Fantuzzi missed the crux of Shirai's argument. In what follows, we will explicate this point, and will then reply to some of Fantuzzi's criticisms of connectionism in general. All of the replies will presuppose that the reader has read Shirai (1992) and Fantuzzi (1992). We will also take this opportunity to briefly discuss the role of connectionism in constructing a general theory of second language acquisition.

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A REPLY TO FANTUZZI

Fantuzzi (1992) attempted to accomplish the following two goals: (1) to criticize Shirai's (1992) discussion of L1 transfer within the connectionist framework, and (2) to present weaknesses of connectionism in general.

Fantuzzi's criticism of Shirai was based on the assumption that his discussion was primarily at the level of implementation/instantiation. She argues that "the details of implementation ... are not given, and many other questions remain unanswered . . ." (Fantuzzi, 1992, pp. 321-322). However, Shirai's discussion was, in fact, at a general level.

A careful reading of Shirai (1992) would reveal that the general tenor of his interpretation of SLA findings within the connectionist framework cannot be viewed to be at the instantiation level, particularly since the purpose of his paper was clearly stated in the abstract.

> The purpose of the paper is twofold: (1) to comprehensively discuss conditions under which L1 transfer tends to occur, and (2) to explain these conditions in terms of the connectionist *framework* of second language representaition, processing, and acquisition, primarily relying on the localized connectionist model ... of Gasser (1988) (Shirai, 1992, p. 91, italics added).

In addition, Shirai stated that he "will discuss L1 transfer in such a way as to be congruent with both localized and distributed approaches" (p. 113). This clearly shows that his discuss of transfer was at the level of a general connectionist framework, not at the level of particular instantiations.

Fantuzzi (1992) further suggests that Shirai (1992) claimed radical connectionism. Although Shirai did claim that connectionism is a radical shift from the traditional symbolic paradigm, he did not claim that "radical" connectionism, among the various types of connectionist models, is *the* right approach. This is clear from the fact that: (1) he did not claim any superiority for either localist or distributed representation (p. 95), (2) he suggested that a promising way to proceed would be to adopt a hybrid approach (p. 114), and (3) he discussed the role of

innateness/prewiring in connectionist modeling (p. 103).[1] Moreover, such radicalism was not an issue in Shirai's paper, since it was an attempt to consider connectionism as a *general* theoretical framework for language acquisition research.

In sum, Fantuzzi wrongly assumed that Shirai was making very concrete and specific (i.e., microstructural) claims regarding connectionism and transfer. Indeed, she concludes with the statement that "Shirai's claim for a connectionist explanation of transfer is greatly overstated" (p. 337). Such a conclusion could have been avoided if Fantuzzi had recognized that Shirai's paper was in essence an attempt to explore a connectionist interpretation of the phenomenon of language transfer and, by extension, an attempt to consider a global theoretical framework that could be *general but cohesive enough* to integrate the various findings that have come out of studies in second language acquisition research.[2]

Fantuzzi's criticism of connectionism in general, on the other hand, should be welcomed since it provides the reader with a highly readable summary of the standard arguments against connectionism. However, as Shirai (1992) noted, the debate is still going on. For interested readers, we list a few examples of the more recent connectionist counter-arguments: Bechtel & Abrahamsen (1991, especially Chapter 7), MacWhinney & Leinbach (1991), Plunkett & Marchman (1991), papers in Davis (1992), especially Seidenberg (1992), Churchland (1992), and Horgan & Tienson (1992).

Although we refer the reader to original sources for a more detailed discussion of the debate between the connectionist and symbolic camps, we would like to discuss some points raised by Fantuzzi that are of possible interest to second language researchers. The first point concerns Fantuzzi's claim that language involves "higher-level" functions which cannot be handled by connectionism. The assumption, shared by many researchers, is that connectionism is suited to lower-level functions such as perception and memory retrieval, but not to higher-level mental processes such as thinking and reasoning. Currently, however, connectionists are trying to see how far they can extend connectionist applications to encompass higher-level cognitive processes (e.g., Rumelhart, 1989), and there has also been a sizeable increase in the number of publications on connectionist research in language in recent years.[3] This indicates an encouraging and healthy trend in cognitive science, and by

extension in theoretical and applied linguistics, since the postulation of numerous new research questions tends to increase the likelihood that some interesting (and perhaps surprising) new answers will be found. Evidently, it is still much too early for researchers to limit the capabilities of connectionist networks, or to consign their role only to the simulation of lower-level functions.

The second point that needs to be discussed is Fantuzzi's use of the term "vague" in criticizing Shirai (1992) and connectionists. One example is her criticism of Seidenberg and McClelland (1989), in which Fantuzzi states, citing McCloskey (1991):

> While Seidenberg and McClelland have provided an explicit computer simulation of a cognitive behavior, McCloskey argues that the underlying theory of human cognition remains vague (Fantuzzi, 1992, p. 328).

This statement, in fact, highlights an important issue of the limits of explanation and description. As Seidenberg (1992) points out, the phenomenon of spelling-sound correspondence which was simulated by Seidenberg & McClelland (1989) is beyond precise description by categorical rules. Indeed, it has so many exceptions that it can only be captured by "soft-laws" (Horgan & Tienson, 1992). Such systems that evade precise characterization are numerous in language as well as human cognition. If one works within the traditional symbolic approach, such systems have to have two processes, one for rule-based items and one for exceptions (which have to be learned by rote). Now, for such systems that cannot be adequately handled by rules, it is impossible to predict a precise pattern of correspondence. Therefore, the only possible result is something vague. This is exactly the limitation of the classical/symbolic approach, while connectionist networks handle such systems much better (see Pinker, 1991).[4]

The point here is that one might have to tolerate a degree of vagueness at the level of description if the phenomenon itself is vague and messy, which is often the case with human cognition. Gasser (1990) states that "Once we are willing to accept the possibility of an adult system in which redundancy is rampant, concepts are fluid, metaphor is a fundamental process, and

exceptions are the rule, our picture of the learner and our research strategy changes dramatically" (p. 196). To always expect precision may be misguided since the phenomenon to be described often resists precise description. As MacKay (1988) points out, it is precisely because theories are "flexible and general" (p. 561) that they can account for a wide range of observed phenomena. In other words, theoretical explanations can be "vague" (in the sense that they make general statements rather than precise descriptions/explanations), if they offer other advantages such as elegance, consistency, and "making sense"-ness (MacKay, 1988).

Fantuzzi (1992) also stresses the limited neural plausibility of connectionist networks. We do not disagree with Fantuzzi in this regard. Indeed, we accept that the neurally-inspired connectionist simulations undertaken thus far are still only humble attempts at "neuralness." Moreover, in saying "*to some extent*, the way connectionist learning operates is constrained by neurobiological reality," Shirai (1992) did indicate an awareness of the limits of a connectionist network's "brainlikeness" (p. 93, italics added). The important consideration, however, is whether an approach must strive for neural plausibility, or whether it can be allowed to disregard this criterion, as is often done by functionalists (e.g., Fodor, 1975; Lycan, 1991).[5] On this issue, the debate has also yet to be resolved, but for arguments on a neurobiologically constrained theory of mind, see Churchland (1986, especially chapter 9), Churchland (1992), and Jacobs & Schumann (1992).

Fantuzzi (1992) further claims that connectionist models cannot handle stages of development[6], stating:

> Gasser (1990), for example, explicitly points out that connectionist models cannot yet model "stages" of acquisition, or environmental factors or monitoring, and it is unclear how they could (p. 321).[7]

There are, however, more recent developments in connectionist modeling, some of which capture stage-like phenomena. A recent connectionist simulation worth noting, for example, is Elman's (1991a, 1991b) simulation of incremental learning, in which he simulated environmental change by manipulating the input, and also possibly the internal neurobiological changes associated with

memory capacity by manipulating the architecture of the network (see also Plunkett & Marchman, 1991; Shultz 1991). More specifically, Elman's (1991a) simple recurrent network shows how the network, using an artificial language, learns to produce sentences that are as complex as *The boys who the girl chases see the dog.* In one simulation, he did not change the quality of input all the way through the simulation, in which case the network did not learn. In another simulation, however, he first restricted the input to simple structures (which could represent the less complex nature of early caretaker speech, as well as foreigner talk) and then he gradually increased the number of complex sentences. This time, the network successfully learned both simple and complex sentences.

In yet another simulation, Elman deprived the network of part of its memory at the beginning of the simulation, then gradually increased the memory size, by manipulating those hidden units of the network that are responsible for memory and generalization. This time, even though the input condition was held constant as in the first simulation, the network learned complex sentences successfully.[8]

The most important finding, in our opinion, is the result of the simulation in which the importance of simple input at the early stages of development was demonstrated. If children have a learning capacity comparable to a connectionist network, which is very likely, they can learn complex sentences successfully if given simple input at the beginning. They will probably create a prototype based on the simple input, and generalize it to more complex/varied situations.

Elman's distributed modeling of complex, hierarchically organized syntactic information is a good example of how environmental factors and developmental change (or "stages") in language acquisition can be simulated via a connectionist network.

Finally, to counter the argument that connectionism is merely another form of symbolic implementation, an argument often referred to in the literature as Fodor & Pylyshyn's (1987) "connectionism as implementation" argument, connectionists typically argue that connectionist models are able *not only* to account for phenomena that can be easily captured by symbolic models, *but also* to account for phenomena that *cannot* be

effectively explained by them. Bechtel & Abrahamsen (1991), for example, argue as follows:

> The connectionist goal is to achieve models that give an account of the phenomena that are handled rather well by rules but also, without additional mechanisms, give an elegant account of other phenomena as well (e.g., learning and variation). *If connectionist accounts did nothing more than implement what traditional rules already do well*, they probably would not be worth the effort involved in constructing them (p. 217, italics added).

With regard to the point made above, it should be stressed that traditional symbolic models are not good at handling "soft-laws" (as discussed earlier), while connectionist networks are excellent at doing this. For example, connectionist networks are good at handling language phenomena that prove awkward for symbolic models, among them the spelling-sound correspondence of the English language discussed earlier (Seidenberg & McClelland, 1989) and the acquisition of German declensions (MacWhinney, Leinbach, Taraban & McDonald, 1989).

In sum, Fantuzzi's criticism of connectionism can be countered in many ways, some of which we have presented above. Of course, it is also possible for Fantuzzi and others to further counter some of the arguments presented here, and that is precisely our point: the debate is still on-going. What is needed on all sides is a spirit of openness that is conducive to scientific inquiry.

CONNECTIONISM AND THEORY CONSTRUCTION IN SLA

Recently, there has been increasing interest in theory construction among second language researchers. Following the publication of McLaughlin's (1987) and Spolsky's (1989) books on theories in second language learning, three symposia/conferences[9] on SLA theory have been held, and papers from these conferences have been (see Spolsky, 1990) or will soon be published. (See also Beretta, 1991 and Crookes, 1992 for

additional discussions of SLA theory.) Here, we will discuss the role that connectionism might play in theory construction in SLA.

One of the criticisms often raised of cognitive psychology is that although it has accumulated a great deal of data through empirical research, there is no theory as yet which enables us to make sense of all the data in an integrated way (MacKay, 1987, p. xiv). MacKay (1988) attributes this situation to the predominance of "empirical epistemology" as opposed to "rational epistemology." According to MacKay, under empirical epistemology theories will emerge after the accumulation of enough empirical data, whereas under rational epistemology this "critical mass" notion is rejected and theories instead emerge as "inventions, products of cognition rather than observation" (p. 561). MacKay's (1988) point is that to construct a theory, a researcher should not be bound strictly by empirical epistemology, but should instead strike a balance between the rational and empirical epistemologies.

Turning to SLA, the picture looks quite similar. The areas of investigation in this field are so broad, the approaches so varied, and the purposes so far from uniform that Long (in press) was able to come up with a list of 29 SLA theories. These differ in many dimensions such as form, type, source, and scope. It appears almost impossible to come up with one theory/framework that can account for all the empirical data.

Connectionism may contribute to the formation of an integrative theory to explain the various findings in SLA research. At the general conceptual level, connectionism can explain a wide range of phenomena. As MacKay (1988) claims, a small number of theoretical constructs such as nodes, activation, connections, and hidden units can account for a large number of empirical facts. This helps us "make sense" of a wide array of observed SLA phenomena that appear to be unrelated.

One might wonder if such general statements have much value as theoretical statements. However, MacKay claims that under rational epistemology, quantitative statements are not essential. He states that:

> quantitative expression is desirable but not essential for theoretical terms under the rational epistemology. Qualitative statements describing how hypothetical constructs such as nodes relate, interact, or change over time,

> in the absence of mathematical descriptions or simulations of these phenomena, still qualify as theoretical rather than empirical statements (MacKay, 1988, p. 560).

MacKay also adds that in the history of science, progression from qualitative to quantitative expression of theoretical concepts is the norm, thereby suggesting both the need for general conceptual statements at the early stage of theory construction as well as the need for subsequent quantification/formalization. Connectionism, of course, has the potential of formalizing its theoretical notions by means of neural network computer simulations. This often makes it possible for somewhat vague statements at the general conceptual level to gradually attain greater specificity (Yap, 1992).

In other words, there are many advantages in promoting connectionist explanations/conceptualizations in SLA (see Shirai, 1992, pp. 93-94) since, by using a general connectionist framework, the relationships among what appeared to be unrelated phenomena can be interpreted in an integrative fashion. This is the thrust of Shirai (1992). In addition, by introducing a connectionist metaphor, it is possible to bring back more emphasis on teaching/learning in second language acquisition theory since connectionism focuses on learning (i.e., representational change, see Bates & Elman, 1992 and also Hatch, Shirai & Fantuzzi, 1990).

Although in Shirai (1992) the application of the framework was limited to the phenomenon of language transfer, connectionism may be broader in its application to language acquisition. For example, Yap (1992) discusses how Bates & MacWhinney's (1982; 1989) Competition Model is now reinterpreted within the connectionist framework (see MacWhinney *et al.*, 1989), and argues that Andersen's (1988) Cognitive-Interactionist Model can also be reinterpreted within a connectionist framework and can possibly be implemented by computer simulation. Both of these models are much broader in scope than the phenomenon of language transfer that was discussed in Shirai (1992). In view of the call for a general theory (Spolsky, 1989), it is important to explore how connectionism can contribute to theory construction in SLA.

CONCLUSION

We would like to conclude this paper by suggesting the direction of possible future research within a connectionist framework, although this overlaps somewhat with what has been discussed in Shirai (1992). First, at the general conceptual level, we should try to reinterpret existing findings in SLA from a connectionist perspective. This will allow us to roughly estimate the scope of connectionist research in SLA. Based on this speculative theorizing, we can then start actual network simulations in an attempt to quantify and formalize our qualitative theoretical statements.

In summing up her criticisms of connectionism, Fantuzzi (1992) concludes that "connectionist models will probably never replace higher-level explanations in cognitive modeling" (p. 337). Although Shirai (1992) never claimed that connectionism would replace the existing symbolic enterprise, he did suggest the possibility of a paradigm shift and, indeed, it might already be on its way. Ramsey, Stich & Garon (1991) claim, "There is no question that connectionism has already brought about major changes in the way many cognitive scientists conceive of cognition" (p. 199). One major change has already come from the symbolic camp. Pinker (1991) and Pinker & Prince (1991) have argued for "a new approach" to morphology, which assumes both a rule-based symbolic representation for regulars and an associative memory for irregulars. Departing from the traditional symbolic approach, Pinker & Prince (1991) argued:

> The conclusion we draw is that generative theories are fundamentally correct in their characterization of productive rules and structures, but *deficient* in the way they characterize memory of less predictable material, which must be associative and dynamic, somewhat as connectionism portrays it. It is necessary, then, to develop *a new theory* (p. 233, italics added).

If Pinker's claim is correct, then the next question we need to ask is: To what extent can we apply productive (or symbolic) rules to explain human cognition? If, as connectionists argue, such areas are minimal, and most of our cognition is in fact organized by

"soft-laws" rather than "rules," a paradigm shift may actually be in the making.

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NOTES

[1] To elaborate on this point, Fantuzzi claims that "radical connectionists typically argue that all learning is based on the processing of input, and that there is no need to posit any a priori internal structure to the processing system at all" (p. 319). If this is one way of viewing "radical" connectionism as defined by Fantuzzi, Shirai's (1992, p. 102) discussion of innateness/prewiring in connectionist modelling clearly shows that Shirai cannot be a "radical" connectionist.

[2] Although it is a minor point, Fantuzzi's (1992, p. 336) discussion of CA (Contrastive Analysis) is also problematic. She presents Gasser's (1990) simulation as if it goes against Shirai's (1992) claims about CA. However, the two are totally compatible. Gasser's point, as presented by Fantuzzi, was that transfer appears to be a complex phenomenon involving the interaction of numerous variables, and this is exactly what Shirai claimed (p. 111); Shirai's point was that CA is only one of many factors that determine transfer. Also, Shirai was not arguing for "traditional" CA, although this was what Fantuzzi implied.

[3] Gasser (1990) reviews a number of recent simulations in the area of language representation, processing and acquisition, while Gasser and Lee report a simulation on phonological feature persistence (Gasser & Lee, 1990) and another simulation on morphophonemic rules (Gasser & Lee, 1991). More recently, Gasser (1992) reports a network model on syllable structure. Harris (1990, in press) reports on the representation of polysemous structures in lexical semantics, while Elman (1990, 1991a, 1991b, 1992) reports on recursive network simulations on the acquisition of grammatical categories and long-distance dependency (e.g., *The boys who the girl chases see the dog*). The list is far from complete, and is in fact growing. (See also papers in Sharkey , 1992.)

[4] Pinker (1991) argues that there are two fundamentally different processes involved in the acquisition/use of past tense morphology; one for symbolic rule-based memory (to deal with regular verbs), and the other for associative memory (to deal with irregular verbs), the latter being most likely handled by a connectionist-like network.

One wonders, however, whether these two systems are totally distinct, or as Harris (in press) suggests, whether there is not, in reality, a rule-analogy continuum.

[5] It is interesting that Fantuzzi (1992) criticizes connectionism for lacking neural plausibility when, in fact, most symbolic modelers regard neural plausibility as unimportant. In any event, connectionist networks have more in common with real neural networks than symbolic models do (M. Gasser, personal communication, March, 1992).

[6] Another counter-argument to the claim that connectionism is unable to handle "stages" is made from a methodological standpoint. It is suggested (see Bechtel & Abrahamsen, 1991; Schmidt, 1988) that what appear to be "stages" are not that clear-cut, and in fact it is "stage-like" behavior that language acquirers exhibit. It may be the case that the symbolic approach, which tends to assume distinct stages, has imposed stages where there are none. In SLA, for example, Andersen (1991) questioned the four-stage negation continuum (e.g., Cazden, Cancino, Rosansky & Schumann, 1975) which is assumed to exist, and argued that the development is not as clear-cut as the four-stage model suggests.

[7] M. Gasser (personal communication, February, 1993) has since updated his views on this issue and now agrees that connectionist networks do have the capacity to capture "stages" of acquisition.

[8] This simulation is also interesting in that it suggests that memory size is a possible reason for the critical period observed in second language acquisition. That is, children's limited memory may be an important condition for successful syntactic acquisition (see Newport, 1990 for a similar account of the critical period).

[9] They are the Colloquium on the Scope and Form of a Theory of Second Language Learning held at 1990 TESOL in San Francisco, the Conference on Theory Construction and Methodology in Second Language Research held at Michigan State University in 1991, and the Workshop on Theory Construction in Second Language Acquisition held in Washington, D.C. in 1992.

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Yasuhiro Shirai has a Ph.D. in Applied Linguistics from UCLA, and is now an assistant professor in the Department of English at Daito Bunka University in Tokyo, Japan, where he teaches linguistics, second language acquisition, and EFL. His research interest includes crosslinguistic acquisition of tense-aspect morphology and cognitive models of L1/L2 acquisition.

Foong-Ha Yap holds MA degrees in English and TESL, from Loma Linda University and UCLA respectively. Currently she is English panel chair at Tai Po Sam Yuk Secondary School in Hong Kong. Her research interests include cognitive models of language learning, and neurobiological perspectives on language acquisition.