To: Licklider @ ISI
From: Newell @ CMU-10A
Date: 16 Apr 75
Re: AI ROAD MAP MEETING
CC: Carlstrom @ ISI, Fields @ ISI, Russell @ ISI, Simon @ CMU-10A
JMC @ SU-AI, LES @ SU-AI, CCG @ SU-AI, PHW @ MIT-AI,
Feigenbaum @ ISI, Amarel @ ISI, Nilsson @ SRI-AI,
Sacerdoti @ SRI-AI

Lick: Twenty-four hours has permitted some reflections on Monday's session and the problems attendent thereto.

(1) To repeat what was generally accepted, implicitly and explicitly, by the AI scientists at the meeting: It is appropriate, given the current general context and specific ARPA context, for the AI field to attempt a series of applications. Such applications would be a good thing for AI generally, as well as for its specific relations with DoD.

(2) Also to repeat: Application opportunities must be discovered, verified and exploited. Some institutional means must be found to expedite this. For it is clear that the AI community by itself does not have the expertise nor the connections to find high payoff activities. Dave Russell, at the end of the day, strongly posited a mechanism of a Rand-like agency with the mission of finding application opportunities, verifying them, building a bridge to the AI Labs, etc. There would also exist, as an adjunct to this, an AI Applications Technical Group (or some such title), consisting of representatives of the various Labs, CMU, MIT, SRI-AI, SU-AI, SU-HP, plus maybe others in related programs, such as BBN-SUS, SDC-SUS, Amarel, etc. This group would be a prime forum and initiation point for these applications. I expressed some concern that such an agent could come into being in short enough order to satisfy the needs of the day (implying that some temporary vehicle would have to be erected), but Russell seemed confident that such expedients were unnecessary. It would be better his way.

(3) It is extremely important to be sure that the payoffs of a specific application are real. It is too easy to get sandbagged to have a seeming application turn to dross. Given that ARPA is prepared to spend large fractions of its AI community (a relatively precious resource) on producing some specific applications, it is critical to substantiate the need and acceptability of an application. ARPA itself, though inside the DoD and much closer to the application sites than the AI community, does not itself have the expertise and, importantly, the time to examine the situations enough to make

reliable assessements.

(4) To be concrete, on reflection I am not at all convinced that the items on Heilmeier's list are all really in the category of genuine application opportunities. I cannot speak about the ASW problem, since that is not a single problem, but many -- namely, the question of where to apply AI techniques all up and down a complex system. But the other two are bitty problems aimed at highly specific targets. I did not get any sense that APRA really knew in detail whether the payoffs were real or simply ephemeral, momentary opinions of one or two high level people in the organizations connected with the applications. Heilmeier's carriage-trade philosophy requires a really good marketing and marketing research arm if it is to succeed. It will do ARPA no good if it squanders its substance on a bunch of irrelevant mirages. The military scene is littered with the dead bones of expensive solutions which were only monuments to someone's folly.

(5) It appears that MIT-MAC is already spending substantial sums (relative to the apparent size of the problem) on the Morse-code problem. Surely it would not be fruitful for the AI community to get further involved in that one. I am not quite sure why this one showed up on Heilmeier's list, but maybe it was just meant to be illustrative of what ARPA had no decided to do.

(6) On the language-spotting task, I need to reiterate what I said at the meeting. First, I do not think the AI Labs (in counterdistinction to the SUS Labs) should take on the problem, they simply would have to build up much of the expertise that the SUS Labs have, which would be a genuine duplication. Second, I consider that the SURG has not been asked to consider taking on that task. I agree that you possibly mentioned it to the SUSC (though I have forgotten it completely), but since it was not brought up in the context of a serious confrontation with the SUS 5-year goals, I do not take it as a serious proposal. It would have been derilict of any SUS group to take on the task, given how tightly the SUS program is strapped down to the 5 year goals.

I do believe we can consider this task and, if it is important enough, we can consider folding it into the present contractors in some way. But we do have to face the potential effect on the 5-year goals and to see how to work around them. As chairman of the SUSC, I am quite willing to go around on that issue, but I need a signal from you or Dave Carlstom that indicates you want to do that. This is a serious point and proposal I am making, since I do not want to be accused later of having fiddled while Rome burned -- of having not picked up this problem when it was important to do so. I would like some feedback on this specific matter. The problem itself seems rather straightforward, given the current art. I would estimate half a man year for the technical work, if done at CMU given all the facilities. To this must be added the whole custumer interface, which might be as much again. Much depends on details which I do not know, of course (eg, how must it be packaged and how much must it cost). If the problem could wait until after Nov76, you

could surely get it taken on by the SUS Labs if it were as important as Heilmeier stipulates.

(7) Another example of a SUS-related application is the Korean on-line communication aid, which you raised as a problem and which I suggested a solution approach to some time ago. I do not know what became of that. Again, it would be derilict for the SUS community to fold that in without at least explicitly facing the 5 year goals. All this stem, of course, from the fact that we (read: ARPA and possibly Newell-cum-SURG-initiator) wanted a program tightly fixed on impressive goals, and therefore not with much slack for such things. Again, I am willing to consider this.

(8) I cannot believe that the CBC is not on the track of an important application. It has two things wrong with it: (1) there is not an immediate customer eager and ready to pay; (2) notwithstanding SRI's search, there does not seem to be one hiding out there quite yet (though I do not know how intensive that search was or is). Yet, it does not seem to me profitable -- for ARPA, even on its own current terms -- to jerk that effort up at the roots and radically redirect it. Rather it seems to me critical to widen the scope to "Real-time operations consulting" (namely, how to help someone carry out an operation in real time) and to search for applications within this wider sphere. The core of work on the CBC remains in fact strongly relevant; and the new applications can be grafted on.

(9) What should go in the Road Map? It seems clear to me that the Road Map for Friday has its action component defined independently of its substantive component. To wit, the formation of the application-finding mechanism, defined above, will not be justified, nor require justification, from the a statement of the current art or a statement of future scientific goals. However, this application proposal will differ from all other such attempts by the promise, implicitly extracted at the meeting, by the AI Labs to enter into such an application-search wholeheartedly.

The substantive mode must perhaps still be there by Saturday. You don't have much to work with, in terms of what was generated before and during the meeting. Thus, I would attempt to get the action component to stand in for the rest. Let me discuss each of the substantive components a little, and then come back to this.

(10) When a set of the worlds best scientists, being asked about the their very own scientific domain, becomes tongue-tied and produces answers unsatisfactory in a first year qualifier, then the conclusion is not that the science doesn't exist, it is that the question was posed wrongly or the situation inhibited adequate response. You asked us, I think, to do something under constraints that communicated: (1) that none of our prior attempts was to be considered satisfactory -- that something new and different was required; (2) that we adopt a form of specification of results and of expectations that is foreign to CS and AI, and largely foreign to science (namely, to state in

advance the content of the scientific results to be expected up to several years in the future, so that the questions would only be whether or when the result would be attained. This pre-empts the science and leaves us tongue-tied).

For instance, in the CMU proposal I have just finished writing a statement about the basic scientific questions of AI and the high level propositions that characterize what we have found out in AI. Apparently that is to be discarded as not adequate or appropriate to the task -- and I am to find yet another statement, different from that, that is to be adequate to the new (yet identical) task.

For instance, Nils has just finished writing a paper (IFIPS 74) devoted to a summary of what AI has done and what areas it has worked in. Apparently that is to be discarded as not adequate to the task -- and Nils is to find yet another statment, different from that, that is to be adequate to the new (yet identical) task.

Let me strongly suggest, for instance, that as far as characterizing the present state is concerned, you take a copy of Nils IFIPS paper and underline in red the items in the bibliography that are done in the ARPA AI Labs, and on the many charts that draw a map of the area, circle in red these same items. This will give (1) a direct picture of the coverage and scope of the field of AI that ARPA has given birth to; and (2) a direct picture of the extent to which ARPA is responsible for these results and for the important ones.

Let me further suggest that you put in front of Nils paper the first section on AI goals from the CMU proposal, as giving a high level coherent picture of what AI as a science is striving to achieve and what in global terms it has found out.

These two items answer only the question: What are the results in AI in its own scientific terms. They do not answer it fully, but they will do as well as what you can put together in yet one more attempt in a few hours.

(12) I do believe that several additional descriptions of AI scientific results are possible that will appear to be more satisfactory to upper ARPA than the two above items (if, indeed, anything is satisfactory). I cannot carry out these descriptions in the time available, indeed I think it would take a couple of months of very hard work (maybe more). But I can sketch and illustrate one part of it (which is indeed based on past efforts to systemitize).

Progress in AI proceeds in terms of increases in scientific knowledge about the various components of the intelligent agent, components that are defined functionally. A standard division, which corresponds in part to Nilsson's core areas, is:

> Recognition and description (Perception)
  > Vision
  > Speech

- > Language
- > Representation
- > Problem Solving Methods
- > Control Structure
- > Assimilation & Accomodation (Learning)

Within each component one can describe a series of structures (or mechanisms) that are possibilities for this component. The discovery of each such structure and mechanism is an advance for AI and a result. Verification, of course, is required; it comes, usually, from incoroporation in several total systems. Knowledge about each mechanism grows with experimentation and theoretical sharpening. Such knowledge, again when verified experimentally, constitutes scientific results for AI. It consists mostly of statements of adequacy or sufficiency in specific task environments.

Thus, the statement "What are the results of AI" at a given date is a listing of the various mechanisms (usually described by conventional technical names), plus the associated statements of adequacy. This list grows over time, and it, rather than a parametrization of how good are the systems that can be produced constitutes the core transferable knowledge of AI. This core is indeed transferrable, precisely because it consists of the abstracted mechanisms which have been shown experimentally to be useful in several task environments.

I cannot produce the lists of results for the total field, mostly because they have not been extracted, labelled and organized in this way. I can do it for one subpart, that of problem solving methods. Here, much that we know can be given by specific methods (analagous to the methods of numerical analysis). A fairly good list is:

- > Generate and test
- > Hill climbing
- > Heuristic search
  - > Search stragegy:
    - > Depth first, Breadth first, Best first, Progressive Deepening
  - > Evaluation
    - > Evaluation functions, level of aspiration, duplication avoidance, external limits
- > Matching
- > Hypothosize and match
- > Means ends analysis
- > Substitute & eliminate
- > Range restriction
- > Abstraction planning

To find a short way to say what we know, eg, about Hill Climbing, takes more energy than I have at this wee hour. We do know the major things to beware of (Multi-modality, Mesas, Ridges, Cliffs), we do have some empirical things to say about when Hill Climbing seems to work and when it doesn't. We do have a way of classifying the refinements of the method (as simplifed models of the hill, which are used to predict the

optimum hill-step to take). And so on.

The existence of this list implies a large kit of tools available to be used in applications, and indeed, when Ed Feigenbaum says they used "standard AI" in Dendral, he means that the techniques in Dendral pretty much are drawn from the list of such own and characterized methods.

To plot the success of the Problem Solving Methods component over time is to watch this list grow and/or the amount of knowledge about each such component grow.

I do not know how much each of the other components can be so characterized, though I expect it could be pushed quite far. But not tonight!

(13) Future goals of AI must be in general to extend the mechanisms and structures of each area and to show that they are adequate to wider and more difficult problems. The discover of a new method is not to be asserted in advance as a goal, if it could then the new method would have been found! Sometimes one knows enough about a method or structure to specify as a goal that it is to be explored. To find the true scope of the range restriction method, is such a (small) goal.

Goals, in the sense that you (read: ARPA) want them, are only to be associated with systems or with instruments (Physics has such goals for the energy of interactions its accelerators will reach, or the resolution of microscopes). There has not been any difficulty, as far as I know, in determining how to parametrize the structure and performance of specific narrow classes of systems (such as SUSs or Dendral-like systems) when the task comes close enough to feasibility to make it worth adopting a system as an AI goal. It can surely be done for various other specific classes of systems, though it will not cover, thereby, all of AI's goals.

[I have run out of gas here -- I believe more can be said about stating AI pure goals, but it just escapes my fog-bound mind. I move on.]

(14) Applied goals, I believe are to be stated in one of two ways. These provide other ways of describing what AI has done in terms of how it can contribute to these goals.

One way is how we started out to do it at the meeting. An applied system is posited (ie, a total military system, such as an ASW system). Then, within that some points of AI application are found, from which one attempts to derive the AI that might make a difference. This is a form of systems analysis, and one that can lead to a backward chain of available relevant research results and, as well, of still needed research -- methods, structures, knowledge, experimentation, , etc. needed to do the job. I think we should do a substantial amount of this, and I believe quite satisfactory road-map results would come out of it. Unfortuneately, I believe that the effort per complex total system must be a summer study-group sort of thing, ie,

about what we put into the SUS initial report, But this is exactly what is to be done by this Rand-like agency (plus some of us),

The second way is to specify applied technologies, The two prototypic examples at hand are the notion of a SUS technology and (more pertinent) the natural language front-end technology that we all were talking about, One can take the development of such a technology as a goal and describe both what existing AI already provides and what new research is needed to get it, This can be carried out much more within the AI community, though some sense for what is really required to make specific applications go is important, But again, it takes a fair sized effort to lay out such a technological alternative, We could commission such explorations, (It would depend, I guess, on upper ARPA being prepared to consider such expansions,)

(15) We did assert rather strongly that there have been a number of civilian applications of AI, eg, in management science, in design, etc, Ferreting these out and asking whether any of them could be applied to military systems would be an additional important task for this applications organization (along with some of us), This, of course, is yet one other way of stating AI results,

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I am really to the end of my rope tonight (this morning) and I will send this out after Herb gets a chance to look it over, I am willing to work on expanding or modifying any piece of this, I am pretty much around from here through the weekend,

A,N,

P,S, Recall that I am expecting feedback on the Language Spotting issue,
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