PLAND 69-72

.01 Randomness: A finite seq., A, is "ϵ-Random" wrt a gu. UMC, M, if:
$A^\frown\alpha^{(n)}$ all have b costs within $n\epsilon$ of each other, for arbly large n.
.03 Here $\alpha^{(n)}$ is any binary seq of length n.

Some Q's: (a) Can a finite sequence ~~...~~ be ϵ random for arbly small ϵ? (I think not).
(b) Just how much & in what way does t. nature of M influence ϵ-randomness of finite seqs.?

Discn: T. value of t. defn! Actually, t. main reason we want to know why a gu. seq. is random is — can we extrapolate it? — can we find any regys in it?

If there are any serious diffys in t. defns. of randomness using K-complexity — then this mite be because (a) cant t. defn. .01 is "correct" (b) For all Umcs k cost − b cost is unbdd for at least 1 seq. } ~~...~~ (b) implying that using b cost ~~...~~ vs. using c cost for defining randomness can yeald, for certain seqs., essentially difnt. results.

~~It may be that whether~~

Whether a seq. is ϵ-random or not will depend on whether one uses a UUOM or a UMSM to define t. probty.

The defn. of .01 looks Bad!: For any finite seq. A, $A^\frown 0^{(n)}$ {$0^{(n)}$ = n 0's} will have a ~~...~~ b cost ~ indip of n — so no seq. "A" could be "ϵ random" in t. sense of .01 − .03

Perhaps: b cost of $\left[A^\frown\alpha^{(n)}\right]$ = const + b cost of $\alpha^{(n)}$.

01;69.01! My usual defn. of randomness would be that a long seq. is random wrt ~~UOM, M, if~~ UOM, M, if ~~...~~ is bcost is ≈ its length.

.03 At present, t. defn. being used is that kcost ≈ its length (This gets rid of Loveland's objection that info about t. seq. can be coded into t. no. that tells how long it is — since we don't tell how long it is — we just consider/ seqs ← that start out t. way t. corpus does)

~~If there are seqs for which kcost - bcost is unbounded~~

If, for all seqs, kcost - bcost is budd, then t. defns of .01 & .03 are equivt. If ? is not budd., then it is unclear as to whether t. defns. are difrnt. — If they are difrnt, then there ~~will be~~ may be K-random seqs. in which one can compute t. continuation of t. seq. fairly well.

Another problem arrises w. t. introduction of MSM's. Do there exist seqs. that are random wrt. UOM's, yet which have strong regys when analyzed by a MSM? Well — a seq. that is untractable for a UOM, can be very well predicted by a MSM. ~~An untractable seq. looks random.~~ Hvr. ordinarily, a seq. that is untractable to a UOM will be gn. a bcost > its length. It may be possl. to devise seqs. that a particular (& ∴ all) UUOM's would give a bcost ∝ length, by modifying t. method used for untractable seqs.

73TM 74TM
403 -111

L. C.

logarithmic Convergence.

1:186.40: On t. rate of error convergence for full CMI (not aoc.):
If we have a finitely derbable stoch seq. (f.d.ss), t. Σ bcost error is < bcost of dcrn. so Σ error < k.

For a linear regressn model, w. N coiffs, t. error seems to $\to$ N $\log_2 S$ (S is corpus length).

Hvr., it is possl. to have ∞ coiffs for linear regressn — & still have t. seq. not diverge, (be stationary) & still get good predictions! This would seem to conflict with t. results I obtaind in 243.01 – & ~ 277.01 (See 300.01 – 301.40 & 302 for summary of that work)

From 277.20 I get t. idea that t. difference in n cost betw. ~~even~~ ~~M~~, say MM ex ahead ~~...~~ & t. ~~...~~ ncost of generating pam for corpus pam that generated t. seq is

$$\text{const} + S \ln \sigma_T + \tfrac{1}{2}(N+1)\ln S - S\ln\sigma_T$$

$$\approx \text{const} + \tfrac{1}{2}(N+1)\ln S. \quad \text{— Which may} \to \infty \text{ as } N \to \infty.$$

Unless the true express. is realy $c_1 + \frac{1}{2}c_2(N+1)\ln S$,
in which $c_2(N+1) \to$ some const as $N \to \infty$

So this suggests a likely possl. error in t. analysis leading up to t. formula of 277.20

~~It may be~~ e.g. it would seem that very small regressn coifs would tend to have small amounts of expected error. Furthermore, (t. set of coifs, a_i ($i = 1/\infty$) satisfys something like $\boxed{\sum a_i^2 \lessapprox 1}$ ~~which produces~~ (if t. seq. doesnt diverge) — in which case t. limits on t. errors in coifs must also satisfy, a fortiori, $\sum \epsilon_i^2 \lessapprox 1$.

The MS value of t. seq. ~~is~~ very large if $\sum a_i^2$ is close to 1. If $\sum a_i^2$ is << 1 then t. MS value of t. seq. is $\approx \sigma^2$,

so MS seq $\propto \approx \dfrac{\sigma^2}{1-\sum a_i^2}$; At any rate, it is $\propto \sigma^2$ from dimensional considerations:

MS = ∞ (I think) for $x = 1$
= σ^2 for $x^2 \approx 0$

if $x^2 = \sum a_i^2$, then just what t. functional form of MS(x) is, I don't know. $\frac{1}{1-x^2}$ is a guess.

01:403.40 T. forgg. analysis could modify my old results in SM on linear regressn. T. constraint $\Sigma a_i^2 < 1$, (or equivt for non-linear terms) does give a kind of strong apriori on t. coifs.

More specifically, t. constraint is $\sigma^2 f(\Sigma a_i^2) =$ MS value of seq.

.05 – where $f(x)$ is some function that I can compute.

It should be possl. to incorporate .05 into t. soln. of t. Max Method eqs of 1962 → TM8530; 570–590 — Yet it would seem that in this work, I did use all of t. available info on constraints. i.e. I considered all possl. sets of $\vec{a}, \sigma^2$ combinations. If certain ones had Σa_i^2, σ^2 combinations that were not in accord w. t. data, they got very low apsi wt. ← ~~So it may~~ Also note that here its not an exact eq. but only usually this is true. (I should think very low apsi wt.)

Eq. .05 is true for t. more likely $\vec{a}, \sigma^2$ combinations that one considers. Hvr., a subset of those give best predictions, & one is most interested in them.

If one has an ∞ of $\vec{a}_i$ components it would seem more reasonable to use floating pts or some ~ notation (having a more reasonable apriori) rather than normal fxd pt. notation for coifs.

T. discn. of ~~the errors induced in~~ t. ↑ in MS predn error induced by errors in t. coiffs: One discn is 251.20 – .40 & also 244.70 – .40. Here 251.29 – .37 does consider t. effect of Σa_i^2, k as used there, $= f(\Sigma a_i^2)$ so

$k\sigma_+^2 =$ MS value of seq. $= M_{ii}$ $(i = 1/N)$.

Hvr., 244.33 gets t. best error / to use in t. r^{th} coiff is $S_r = \sigma_D \sqrt{\frac{1}{2c_r}}$

.35 σ_D is MS predn. error $c_r =$ MS value of seq. – so clearly S_r is t.

.36 same for all coifs – which is not reasonable ~~..~~ e.g. if N (t. no. of coifs) is large or $\to \infty$. Also, Note 251.28, $\delta E = \sum_{j=1}^{N} \delta a_j^2 M_{jj}$ (total error ↑)

.37 ⟨Actually 251.26! $\delta E = \Sigma\Sigma \delta a_i M_{ij} \delta a_j$ is exact. which is terrible as $N \to \infty$! → 410.28

.38 One possl. explain. (if true): If one uses N coiffs, t. accuracys needed in each, are, indeed, t. same. If there are ∞ coifs that could be used, t. Q is only how many to use, & perhaps which ones. 405.01 As one changes this N, t. optimum accuracy in each coif, as well as t. MS error will change.

01: 40440: Also, another Q about t. analysis in eq. 404.37 — neglecting non-diag. terms. If M_{ji} = some funct of $(i-j)$ only, I think it very likely that one could chose t. individual s.d. errors so that t. off diag terms just about cancelled. If t. errors are uncorrelated, we expect them to cancel for any set of coiff w. $i-j$ = const.

.05 ~~A poss. Explaxn~~ of 404.37 → 410.28

.06 Another Q involving t. growth of Error w. S; In t. latest JACM [→ Oct 1973 pp 687-95 R.P. DALEY] There is near t. end, a paper on tradeoff betw. regular & Kolmog. complexity. It mentions certain kinds of seqns. in which t. Kcost ↑ as $\log_2 S$. but none w. ↑ as $K \log_2 S$. Just what are these seqs?

Also, if one has potentially ∞ coifs — using t. idea of 404.36-.40 — just how does k cost ↑ w. S?

18 Another Q in this General line! Ordinarily, if I were going to compute π, I would decide how much accuracy I wanted, & then use a series expansion, w. each term having t. needed accuracy. Hvr., in many successive approxn methods, one doesn't need to fix one's target accuracy at t. begining. e.g. for $\sqrt{n}$, we use $X_{i+1} = \frac{1}{2}\left(X_i + \frac{n}{X_i}\right)$ to get error$_{i+1}$ ≈ (error$_i$)^2

Bill Gosper has a method of continued fraction expansion [for π or for functions] that gives one more place of accuracy w. each compn. [He doesn't have to decide in advance what accuracy he wants] Perhaps the N^{th} compn. has cost ≈ N.

(For some stuff relevant to Gosper's ideas see 1973 HAC "HACHMEM")

This would seem somehow relevant to t. coding of sequences, — in which one needs more accuracy in t. coifs as t. seq. gets longer. So in this sense, one's coding is not sequencial. Hvr. in Max. Method coding, each point uses a new set of coifs — but they cost nothing (no ad hockness). A new value for t. σ^2 must, be obtaind & this costs nothing either. — what does cost, is t. ~~err error~~ coding of t. errors wrt t. Gausn distribn. w.t. computed σ^2.

D473 LC TM410

of convergence

01:405.40: On t. problem of encoding a continuous source:

Say a "continuous source" is a ddts. Its output is of mean zero, ≈gaussn, with $\sigma_{ave} = \sigma^2$ — but k bit truncation of t. true gaussn variable. This gives us a simple probty distribn. on 2^k discrete possl. outputs. If σ^2 is rational, t. problys will (probably) all be irrational ∴ not finitely computable.

.10 If t. source is a multivariate distribn. (i.e. linear regress, gauss distribns.), then truncated to k bits — if t. coifs are all rational, then it will occasionally take an arbly large no. of bits of knowledge of t. previous values, to compute t. next value. This would seem to mean that t. device creating t. seq. (including t. truncation) would have to have ∞ memory capy!

T. present day theory of how sci observations are created: Something like a multivariate distribn creates values that are then truncated. This MV distribn. includes t. noise inherent in any observation. This is t. picture of Modern Physics — So, if one takes this as a true picture of Modern Physics — it conforms to t. Model of .10 ff ∴ must have ∞ memory somewhere!

Hvr., if it only kept, say, 100 bits of accuracy, it is unlikely that anyone would ever find out it was not ∞ bits!

.18: 405.05: Re: 404.37: Say $N \uparrow$ as $\sqrt{S}$ or some slow funct. of S.

Since from 404.37 $\delta E = \sum\limits^{N}\sum\limits^{N} \delta a_i M_{ij} \delta a_j$; all t. δa's are about = (404.35, .36) & t. M_{ii} terms only are impt, & they are indep of S — so

$\delta E \approx N \, \delta a^2 M_{11}$. Since δE is total error, mean error is only

$\frac{\delta E}{S} = \frac{N}{S} \, \delta a^2 M_{11}$: $\frac{N}{S}$ can $\to 0$, but less fast than $\frac{1}{S}$.

Hvr, δa^2 may get very small as $S \uparrow$ — so $N \, \delta a^2$ mite possibly $\downarrow$ or $\to 1$. If it does not $\downarrow$, there is little motivation for increasing N!

.35

→ This suggests that Fredkin's idea, that all info processes have finite info proc. capy, may be necessary!

(1974)
110.05

~3 · ~~2~~ 74 (log. conv.) TM 110
(1973) Gaun TM
41a40 1) Computable functions can be discontinuous if one stipulates that t. args be expressed as rational fractions or terminating decimals. T. necessity of continuity of computable functs. is an artifact of t. method of representing nos.

.05 2) On t. rate of convergence of Σ t. (error)^2 for various PEMS: (Σ probty error)
a) for finitely describable pem.
b) $\Sigma(p_i - q_i)^2$ is bndd.
c) For t. bern seq. w. probty, p = const & random,
$\Sigma(p_i - q_i)^2 \propto \ln s$ (s ≡ corpus length).
d) for an N param regressn, it is $\propto$ (each param "random") N lns. (tho t. const of $\propto$ depends on σ^2 or on t. Σ of coefs or . . . . .)
e) for ∞ params (each random), it is $\propto$ something > lns. (i.e. there exists no constant ∋ Σ err^2 < k lns.).

,25 3) To compare 2 pems: each w. infinitely long dern:
a) binary Bern seq. w. p = irrational.
,27 b) A deterministic random seq.
These can be represented by a UMC. w. 2 unidirectional input tapes, a work tape & a unidirectional output tape. One input is a finite seq. that dcrbs t. str. of t. seq. T. other input is an infinite string giving t. random value of t. proby in 1 case, & t. random value of t. seq bits in t. other.

.35 3.5) For each PEM, one can write an expected "dern length" for all corpi of length s. Hvr., what I mean by "dern length" here is not t. entire dern. length, but only t. part of t. dern. that dcrbs t. pem. 111.01

01: 110.40 What I want to be able to do, is distinguish betw. t. 2 pems of 110.25. They are clearly difrnt. in their error convergence characteristics. How could I know this from t. derns of t. pems themselves? — since both have inf. long derns.

I think that t. remark of 110.35 mite be explicated in t. follg way: Gn. t. dern. of t. Binary Bern. seq.: All sequences of length ≤ S have a "common" dern portion w. $\propto \ln S$ bits — i.e. this common dern portion is adequate for all corpi of this length or less. While t. general Bern. seq. has this ∞ info content param, p, in it, only ~~[illegible]~~ t. first $k \ln S$ bits of p need be known to code a corpus of length S.

On t. other hand, for t. deterministic random seq. of 110.27, S bits of t. pem dern. are necy to code a corpus of length S.

C 86-89

3974 C (on the constant "c" — the difference between using different UMCs for induction).

TM 86

The fear was raised that the difference ($\equiv$ simulation instructions) between 2 UMCs can be <u>enormous</u>. Just what effect does this have on practical induction?

06 Conjecture: Ordinarily, in practical induction, it has <u>very</u> little effect. Reason: Consider that our corpus is very large. It's in 2 parts ① the large beginning string, A ② a relatively short new string, B: corpus $\doteq A^\frown B$.

Say the simulation bit distance between the 2 UMCs we're using is $\ll$ length(A).

5 → Then, after coding the corpus up to A, both machines are <u>effectively about the same</u>. — i.e. they have both used the same defns, & names of strings, etc. After coding A, they have both used up about as much $\Sigma(\text{err})^2$ as they could.

19

A more exact treatment: say the xln instructions from M_1 to M_2 are $< b$ bits (both ways).

Then $\left| I^{M_1}(A) - I^{M_2}(A) \right| \leq b$.

The mean err² via M_1 & the mean err² via M_2 differ by at most $\sqrt{\left(\frac{2\ln 2\; b}{\text{length}(A)}\right)}$. — which is quite small, because length(A) is very large.

Σ err² via $M_1 = \alpha$ $\qquad$ Σ err² via $M_2 = \beta$

$|\alpha - \beta|$ can't be $> b$ $\therefore$ ...

.30 Note that b can be <u>quite large</u>: e.g. say M_1 is an ordinary

.31 UMC. M_2 discards the first N bits of its input, then acts like M_1 for the rest of the input. N can be 10^{100}, say!

The M_2 of .30 sounds very <u>wasteful</u>! Is there any reasonable way to discard it? Note that M_2 is <u>uniformly worse</u> than M_1. Actually, the machines would give <u>identical</u> proby distribns!

Machines that humans think of as "Reasonable" are machines that express relatively easily the regys of the corpus those humans have known — so those "reasonable" machines are something like

88.01 the machines of .15 that have already coded the enormous corpus, "A".

01:86.40 C

JM88

Note that if I chose a particular Unc, there is probably no problem of very large C. Say I chose an M_1 as 86.30. Then I can ~~[illegible]~~ simulate via M_1, to machine M_2 of 86.31 in $\log_2 N$ bits. Any machine effectively describable by a human, can be dcrbd (& simulated) in not very many bits. So if we chose M_1 for t. standard, M_1 will not be much worse than any machine a human would dcrb.

Why not use Huffman coding for Bern seq? — The thing gives in'l type proby values. Perhaps use Willis' Thrm 5 method!

Furthermore, after practically "any reasonable" corpus of 10^6 bits, this human-chosen machine would be, effectively, about t. same as M_1 — for t. "reason" of 86.06 — .19

13 Note that if we had usd M_1 (86.30) for prediction, we would have obtaind all of the known laws of science — since they are all expressable in not very many bits, & t. amt. of data involvd is enormous. Using the "faith postulate": we note that M_1 seems non-a.h. & it has workd very well in t. past. ∴ we should use it in t. future.

So: I am proposing M_1 as The universal standard for induction. Any other standard for induction ~~[illegible]~~ that is not clearly a.h. — or has no suspicion of a.h.ness — will give almost exactly t. same prediction probys.

Any form of induction that does not invoke the faith postulate should be suspect!

Another point in favor of M_1: we can calculate § very simply, because t. Pem has zero b cost — since M_1 is

.30 → IMPT more or less chosen indply of t. corpus. Hvr., for t. vast hetrogenious mass of R W data, this corpus may not have a usable §. (§ or an average §) The data is in large chunks — each chunk having its own §.

The essential argt. is .13: M_1 is not a.h. & ~~[illegible]~~ in t. past using it has workd very well.

Q: It is concievable that some regy will occur in t. future that has hy b cost wrt M_1. A: If so, we will not be

89.01

01:88.40 able to derv. i.using human beings & present search techniques. TM89

Very probably, all such methods are restricted to using low bcost trial hypotheses.

As for trying ~~sts~~ umc's other than M_1, the dcons of these umc's (wrt M_1) would have to be ~~on~~ t. order of magnitude of t. corpus (this is t. corpus, <u>not</u> t. code or bcost of t. corpus) in order for it to make any difference in the probty estimates.

— This is for t. reasons of 86.06–~~[illegible]~~.19

21374

A More General Solution to the Inductive Inference Problem
and some new, powerful conjectures. 1

The induction problem is defined to be the ~~an~~ probablistic extrapolation of a sequence of symbols generated by an unknown stochastic process. ~~is known to be recursively unsolvable, if the stochastic process is limited only in that it have finite description.~~

If the stochastic process has a finite description and is in a known complexity class, the problem is recursively solvable. If the complexity class is not known, then the problem is Limit recursively solvable in the sense of Gold. ~~These~~ ~~That~~ ~~These~~ ~~solutions~~ ~~devised by Willis.~~ ~~have particularly good rapid~~ This solution converges very rapidly with sequence length, for either of 2 criteria: The first, is bounded information error, the second is bounded ~~is the total squares of the probability estimat~~ ~~of the~~ total squared probability error.

While ~~the~~ statisfaction of the first criterion implies the second, satisfaction of the second need not imply the first.

The forgoing ~~solutions to the~~ remarks apply to Willis' limit recursive solution of the induction problem. ~~that was devised by~~ In the realm of limit recursive solutions however, a somewhat ~~differe~~ more powerful technique is possible ~~...~~ ~~Be able to discover produce the same~~ small error that converges just as rapidly for a wider variety → 2.20

.34 of stochastic sources. A stochastic source can be devised for which is completely untractable by

replace 2.20 by 2.22

Willis' method ~~that which produces the desired bounded error rates~~. The new technique is able to produce ~~the~~ informational bounded error types.

Approximations to the new induction method seem to take much more computation time than corresponding approximations to Willis' method. Thus much as there are no

~~Known cases in the real world in which the new~~

~~[illegible]~~

Whether or not there are any ~~[illegible]~~ stochastic sequences in the real world in which the new method would be superior is certainly ~~[illegible]~~ an open question.

While ~~all of the~~ both of the forgoing induction techniques are neither practically or even theoretical computable, they both suggest approximation methods. Some very strong conjectures ~~have been~~ have been obtained for the expected errors to be obtained using such approximations.

Some ~~formulae~~ expressions ~~will be~~ are presented which are conjectured to ~~[illegible]~~ give a good upper bounds on the error to be expected when any approximate probability evaluation methods is used.

20.134 A stochastic source can be ~~[illegible]~~ described for which Willis' method is inadequate, yet which yields the desired bounded errors when the new method is applied.

.22 Whether there are any sequences of this type in the real world, ~~now~~ is certainly an open (and probably undecidable) question.

While ~~[illegible]~~ neither of the forgoing ~~techniques~~ induction methods are ~~neither~~ practically or even theoretically computable, they both suggest approximation methods. Approximations to the new induction method seem to take much longer to compute than corresponding approximations to Willis' method.

Some expressions are presented which ~~will probe~~ are conjectured to give good upper bounds on the error to be expected when any approximate probability evaluation method is used.

Minsky

Discn w. Marvin. : I said that I took issue w. his defn. of "self-awareness" He thot of this as t. machine's ability to desrb. its own methods of operation — its heuristics etc, to an outsider.

I suggested that "self aware" should apply to ① Recuft. machine w. recuft. extending over > 1 problem, as opposed to ② machines that get recuf. for only 1 problem.

[It seems to me that I had discussd this at length w. him in t. past — yet he seemd unfamiliar w. t. ideas]

He felt at first that there would be no practical difrnce betwt. 2 machine types. I said that t. most noticeable differences would be that a Machine of type 1 would do experiments on its envt. & would try self improvement (i.e. read about electronics, etc). — That if t. ~~total~~ averaging time was N problems, it would have N times as much time to devote to self improvement as a machine w. N = 1 (a type 1 machine).

My impression is that he really didn't get this idea very well — perhaps because it was unpleasant.

Anyway, he said that he felt that t. only way to work very hard problems was to start out w. a tentative soln. & debug it. This now sounds a bit narrow —

Re: The proposal: Winston's thesis uses a "non-universal" lang — so it can't have loops — but this can probly be easily modified.

In my approach to induction, starting from very little info & working up a trng. seq. to any ° of proficiency — I should be careful about comparing it to a child's learning because some reviews will regard this as excessively huberistic!, He felt that a non-experimenting machine would eventually get all this "experimentally available" info by random errors in prodn. Seems unlikely to me that this is true: ~~e.g.~~ the development of Mod. Sci w.o. having done any "expts" that had other than 'directly usable output', is unlikely. I.e. I think much pure experimentation is nec. — i.e. no immediate gain expected.

It may well be that Marv. has forgotten about McCarthy's problem w. the client hypnotizing machine. Also, he didn't see a reinft. mode as being a particularly good way to train a machine.

I think this may be relevant to my criticism of his "Frames" paper — i.e. that he did not much concern himself w. what t. S. was "trying to do" — so there was no way for S. to tell if a particular way he had constructed a frame was a help or a hindrance —

One impt. idea I had was that w.o. a "goal", it was diffclt. to teach a machine what sentences meant. One mite build into t. machine ~~an~~ mechanisms for dealing w. ethnic lang, but it must in addition, continue learning new meanings & nuances of various lang. abss. — i.e. it must "track" t. language.

But anyway, if Marvin is unaware of this difty of open loop T.S.M.'s, it is even more likely that Fredkin is — & I think that my talks w. F. did indicate that he didn't know about t. importance of reinft. machines, as well as their (perhaps) fatal diftys.

3974

General Evolution of A.I. work at **MIT**:

T. Winograd: This is an Enormous pgm. It's General conclusions:

That in order to carry on a conversation about a subject, one must have a knowledge about the subject, as well as some knowledge of English grammar.

This is all in the spirit of Marv's idea that one must know a lot before one can do "Thinking".

However, my impression, is that it is, in effect, an a.h. device to play Turing's imitation game. That it is the sum of y. techniques built into it and it is not ~~partic~~ easy to add more to its repertoir.

Its value is that people will now study both syntax & semantics together, rather than separately

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One of the Big points of emphasis at y. Lab:
How can one build-up an enormous data base that is in a form that makes the data most usable? The current part answer: By putting y. data in y. form of "pgms to do things".

In general, the Lab has worked on getting machines to work problems that humans work, that are regarded as requiring "Intelligence".

Some probs:

1) taking English commands about "block world" & executing them. Conversations about these actions & parts of those actions.

2) **General Techniques of Problemsolving**. The big white hope now is making a poor ruff soln. to a problem — then picking a possl. soln., then debugging it.

3) **Induction**: Winston before him....

I guess my general criticism is this: That they are working on getting machines to solve various dif'lt (for machines) problems, While the researchers may, eventually get fairly good at this — one wonders if there are (devising?) arising any of general principals, so that t. field will grow as a science, rather than a "craft".

Also, if no general principals are found, it seems unlikely that very clever machines will come out of this work.

T. foreg. would seem to apply to almost all of t. work, including the induction work of Winston.

CIAP 74 59-60

2.4.74

Artificial Intelligence for Military Purposes

CIAP:

Paper: Report: Military Applications of contained A.I.
"Contained"

(Contained means: no consciousness. Relatively open loop commands. / can be Used to answer questions. [An uncontained A.I. has closed loop commands with R W/ (including its "clients") contained in its loop. Its optimization criterion involves its integrated action, taken over a period of time. ~~Several of its actions.~~ If one can construct a "contained A.I." it is relatively simple matter to use it as ~~to modify it, so it becomes~~ an uncontained A.I. However, the practical problems of control of the devices -- as well as the necessity for such control, are multiplied unimaginable. (by "unimaginably" - I mean literally; that we are probably incapable of imagining just how much mischief such a device can whip up.) — we are, however, capable of calculating some capabilities for such devices. They are a "mixed bag") I will not discuss this sort of machine further in this paper -]

Scenarios: (1 nation; >1 nation)

The time scale of A.I. (5 to 50 yrs.) ~75% prob? much more certain (10 to 25 yrs fairly likely say 50% prob)

Some proposed solns: Use A.I. to help find a soln:

Rebuttal: When A.I. ~~serious~~ is available to use to help solve its problems, it will immediately be used for Mil. or Meta Military purposes. To use A.I. to help solve the problem, one will have to compete, time-wise & A.I. hardware-wise w. Military applications. At this stage of the game, certainly, using A.I. in a hurry is extremely dangerous.

Artificial Intelligence for Military Purposes.

This report will discuss a very advanced form of A.I. called "contained A.I." "Contained" A.I. means that the machine ~~is given only open loop commands. It~~ can be used to answer questions or can be given open loop commands only. This is opposed to an "uncontained" A.I. — which is given closed loop commands with the real world (containing the "user" of the machine) in the loop. Under very ~~more~~ commonly occurring conditions, such a machine acts very much like an

01 ~~[illegible]~~ extremely intelligent, highly motivated, indipendant living creature. It is very dificult to control such a device. ~~[illegible]~~

The present paper shall deal with contained devices only. They are much easier to ~~[illegible]~~ analyse and give an elementary introduction to ~~[illegible]~~ some of the ~~[illegible]~~ very dificult social problems posed by artificial intelligence.

SN Discn w. Minsky ~3 5 74 (~~[illegible]~~ q.v.)! He doesn't seem to be able to recognize ~~differences~~ that there are impt. differences betw. "contained" & "uncontained" TMs.