Canning 2824, Buenos Aires, Argentin

December 4, 1962

Dear Karl:

Thank you for your letter of Nov. 25. I did not reply to Hennie's letter because I was waiting for good news to come, but only bad news have been piling up lately.

First bad news: The Free Press refuses to publish the book unless the articles by Curry and Davies are eliminated. They argue that they contain much symbolism and drawings that would produce economic losses. I am sure they are right. But they should have said this before signing the publication agreement, which they did last March 22. At any rate, I am desolate, and Curry and Davies will be furious with me. Of course, I have asked them to collaborate, and have taken the liberty of proposing to them that they look around with the hope of finding strong and quick financial support from some institution in order to compensate for the publisher's losses. The net result will be, I suppose, that they will withdraw their papers. For the sake of the success of the project this would be the best, because Curry's paper is much too technical, and Davies' is frankly bad. But, once I had asked them for contributions, I feel I had no right to ask them to withdraw them. I am sure they will take it as an insolence on my part, which I would lament, especially in connection with Curry, whom I like very much. The important thing in all this, though, is the project itself, and in view of this I have surrendered all Latin dignity and pride.

Second bad news: my immigration visa application has been refused "for the time being". Marta and I are bitterly disappointed about this, both because we have to go abroad and because we had already become accustomed to look upon the USA, with all its shortcomings, as our second and, perhaps, definitive fatherland. Our need to emigrate has now become even more urgent owing to the following

Third bad news: the administration of the university has fallen in the hands of Catholic and reactionary people. All teachers with a critical frame of mind are accordingly in danger, and particularly so those who, like myself, will not even be defended by the left.

Fourth bad news: as a former exchange visitor, I am not allowed to reenter the USA until June, 1963. Hence, I will not be able to accept the interesting Texan offer - unless I get a waiver from the immigration authorities. But this is unlikely, especially because the Consulate misled me in this whole question and indicated me the wrong form number to make the application, and now there is little time left.

Our plans are as follows. If I get a good offer, we shall sail for USA ca. August, 1963 and will stay there as long as I am permitted (a maximum of 2 years). Then we shall go somewhere else - if possible not back here, but

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to England. After another 2 year period abroad, we would reenter the USA, and so back and forth as long as I live, ar as long as I cannot be of any help in my own country. The prospect is not too encouraging because, although both Marta and I like travelling, we also like to have a firm base, which apparently we won't have.

The chief personal problem is now to select a good place for us in the USA. And this is far from simple, because 'good' means "best for Marta". Buffalo was OK for me, but the math. dept.there is (confidentially) of a very low level: they specialize in the training of high school teabhers, and their professors teach from 6 to 9 courses a year, all of them elementary. Marta is now in a position to begin some research work in either modern algebra, or algebraic logic, or topology, and a stay at a 3rd rate math. dept. would be fatal for her formation: she now needs a good supervisor. But, of course, if "worse comes to worse", none of these scruples will count and we may have to flee even to Ecuador, which must be, as far as mathematics is concerned, horribly low.

But enough of complaints and difficulties. Aside such negative aspects, there are some good ones. Marta has passed her exam on a seminar on mathematical logic (mainly theory of deducibility and algebraic logic) and is preparing herself for the exam on real functions. I have finished my chapters on explanation and prediction, on which I have some interesting results, particularly on "interpretative explanation" (in terms of nonphenomenological theories) and on the measure of predictive ~~content~~ performance.

An adequate measure of the projective performance of a theory T should be a function of (1) the number of original projections, not otherwise obtainable, and (2) the differences $V(p_i/EA) - V(p_i/A)$ between the posterior and the prior truth values (not probabilities) of the theory's projections (predictions or retrodictions) p_i. I propose the following formula for the projective power $\Pi(T)$ of a theory T leading to the projections p_i that are gauged by the background knowledge A and the empirical procedures E:

$$\Pi(T) = (1/2)\sum_{i=1}^{N}[V(p_i/AE) - V(p_i/A)] \qquad [1]$$

If we decide to assigh as much weight to the new experience E as to the antecedent body of knowledge, we put

$$V(p_i/AE) = (1/2)[\,[V(p_i/A) + V(p_i/E)]\,, \qquad [2]$$

which takes [1] into

$$\Pi(T) = (1/4)\sum_{1}^{N}[V(p_i/E) - V(p_i/A)] \qquad [3]$$

The minimum value of $\Pi(T)$ is obtained when all the N projections are empirically false and, at the same time, they ~~me~~chadict the antecedent knowledge A; in this case $\Pi(T) = -N/2$. The maximum value, N/2, is obtained when all the N projections are found to be empirically true and, at the same time, they

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An adequate measure of the projective performance of a theory T should be a function of (1) the number of original projections, not otherwise obtainable, and (2) the difference $\Delta(b^f_i/E\Lambda) - \Delta(b^f_i/A)$ between the posterior and the prior truth values (not probabilities) of the theory's projections (predictions or retrodictions) b^f_i. I propose the following formula for the projective power $\bigcup(T)$ of a theory T leading to the projections b^f_i that are gauged by the background knowledge A and the empirical procedures F:

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The minimum value of $\bigcup(T)$ is obtained when all the N projections are empirically false and, at the same time, they contradict the antecedent knowledge A; in this case $(T) = -M/2$. The maximum value, $M/2$, is obtained when all the N projections are found to be empirically true and, at the same time, they

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contradict antecedent belief. And for an ad hoc theory, which covers E and sheepishly obeys A, $\Pi(T) = 0$.

The same result is obtained by starting from the idea that the projective power of a theory is greater the more original and, at the same time, the less inaccurate it is. Let us then put

$$\Pi(T) = O(T) - I(T) \qquad [4]$$

Now, the originality $O(T)$ of T relative to A will be given by the differences $V(p_i/T) - V(p_i/A)$ between the truth values assigned on the basis of T and those assigned assuming that A holds. We may then put

$$O(T) = (1/2)\sum_{1}^{N}[V(p_i/T) - V(p_i/A)] \qquad [5]$$

Since $V(p_i/T)$ is near 1, and $V(p_i/A)$ lies between -1 (heterodoxy) and +1 (orthodoxy), $O(T)$ will be somewhere between nearly 0 and N. The minimum of $O(T)$ will correspond to a theory consisting of just a rearrangement of a system contained in A; and the maximum originality value, N, will correspond to a theory which clashes with antecendent knowledge.

In turn, the degree of inaccuracy of T is quite naturally defined as

$$I(T) = (1/2)\sum_{1}^{N}[V(p_i/T) - V(p_i/AE)] \qquad [6]$$

a number lying between 0 and N.

Substracting [6] from [5] we obtain again the formula [3] if we retain the relation [4] between projectivity, originality, and inaccuracy.

Our preference for theories with the highest projective power, then, is a preference for original theories that stand the empirical test. Considerations of probability have nothing to do with all this.

Since $\Pi(T)$ varies with time, it can also be taken as the measure of one dimension of the growth of theoretical knowledge. (This is not the whole story: T may be shallow, i.e., it may have a low explanatory power - in my sense of the term at least.)

Finally, I lay down rules that, I hope, catch the "spirit" of your methodology but make no reference to probability, which I hold to be irrelevant to truth (my main concern). They are:

Rule 1. Select <u>for test purposes</u> those p_i such that $V(p_i/T)$ is high and $V(p_i/A)$ is low.

Rule 2. <u>Adopt</u> (until new notice) those p_i such that, having been obtained with the help of T, have also passed the empirical test - i.e., those with maximal $V(p_i/TE)$.

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