# My connection with cybernetics. Its origins and its future.

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It has been suggested that now, some fourteen years after the writing of my book on cybernetics, I take up the history of the subject and a discussion of the lines of work which seem most interesting for the immediate and the remote future.

I wish to disclaim at the very start any attempt at complete comprehensiveness, for I have neither the ability nor inclination to give an encyclopedic account of all of the ramifications of the subject as a whole. Therefore this talk is going to be highly personal, and will constitute an account of my own relation to the beginnings of the subject and of the directions of work which interest me at present and which seem to me particularly tempting for the future.

My contact with cybernetic ideas goes back to 1919 just after the First World War when I had completed my military service and was looking around for significant problems to which to devote myself in my career as a mathematician. I had read rather broadly in the theory of the Lebesgue integral in Frechet's and Volterra's books on integral equations and the like, and had come to the conclusion that analysis was the branch of mathematics which was most tempting to me, and to which I should devote my career. At that time a colleague, Professor T. Barnett from the University of Cincinnati, happened to be in the American Cambridge, and I asked him to suggest to me what problem would seem to be coming into its critical stage of development. He called my attention to the problems of integration in function space and to the work of Gateaux and to P. J. Daniell. I started following up their work and obtained some minor results on an abstract basis, but these seemed to me thin and lacking in real significance. I therefore asked myself if there were any questions in physics and the other natural sciences in which the integration of functions of curves comes in naturally with a truly physical meaning.

It was in the summer of 1920 which I spent in Strasbourg working with Professor Frechet that a hint of the answer to this question came to me. The problem of the Brownian motion is one in which random assemblages of curves naturally occur. Related to this are a great many problems in statistical mechanics, and particularly in hydrodynamics. My office at the Massachusetts Institute of Technology overlooked the basin of the Charles River, and I had often reflected on the wave pattern of the surface under a wind as another example of a functional entity belonging to a family in which questions of distribution were important. I therefore decided to study the problems of the distributions of functions with this and other similar physical motivations as my basis.

This is not the place to go into the details of my subsequent work which led to a successful theory of the Brownian motion. However two comments are appropriate. One is that I came into contact with the deeply significant work of Taylor, now Sir Geoffrey Taylor, on turbulence in which the notion of autocorrelation played a predominant role. The other is that in applying this notion to problems arising out of the Brownian motion I was forced to study a certain class of functions which had already been studied by mathematicians, but which had been considered as more or less pathological. These were the continuous non-differentiable functions. I found that functions of this sort, far from being nonphysical, belonged to the very essence of the study of the Brownian motion, and of distributions of curves in function space.

My physical interests led me to the physical interpretation of the problem of integration in function space as one of probability. Here I was much influenced by the fact that the ideas of Gibbs in statistical mechanics, after a period in which they had been foreign to the ways of thought of contemporary physicists, were now coming back into vogue and were being really understood. I therefore found myself definitely along the path of investigating random physical phenomena from the probability point of view with the aid of new technical methods which seemed to be precise and promising.

At a very early stage of my work on this subject I became aware that the harmonic analysis of random function was an essential part of my program. I found that this harmonic analysis, although it had been studied from a physical point of view by theoretical workers in optics, and although Shuster had suggested certain statistical aspects of it in his discussion of the periodogram, had been neglected by the pure mathematicians, who, for the most part, had confined their efforts to the study of phenomena which were either strictly periodic, as in the case of the Fourier series, or definitely of limited duration in time, as in the case of the Fourier integral as it was then known. The harmonic analysis of continuing phenomena in time needed a new start, which I began in papers written in 1924 and which I brought to a successful conclusion in my paper on generalized harmonic analysis which appeared in Acta Matimatica in 1930. In these papers my work, which interacted most closely with that of Bochner, made constant appeal to the work I had done on the Brownian motion, and received its most significant applications in that field. Throughout this work I was compelled to consider non-differentiable continuous functions.

From the very beginning of this work, I was influenced by the contemporary investigation of Paul Levy on random functions. It was Levy who called my attention to the fact that my Brownian motion functions had already received a certain amount of study from Bachelier. Bachelier's work, however, while representing an excellent insight, had already been written at a time before the ideas of Lebesgue integration were available for the development of an adequate technique.

My work on random functions suggested to me that I might have a new approach to the problems of turbulence and of statistical mechanics in general. These concepts led me to a new series of papers on the theory of chaos. I found that my work, although unquestionably in the right direction, ran into many difficulties in this field which were intrinsic in the study of the dynamics of random processes. As this is a little off the main direction of cybernetics, I shall not go into them here, except to say that it became amply clear to me that without a really new body of ideas most of the mathematical developments in time occurring in the theory of turbulence and of random processes have the character that the series which one obtains will often tend to have a zero radius of convergence and to be asymptotic rather than convergent. Some recent work of Kolmogoroff has suggested to me the possibility of evading this difficulty, but the turning of these ideas into usable methods is still under way, and it is not yet the time to speak of the processes.

In the beginning of 1930 it had become amply clear that the work of Willard Gibbs on the ergodic hypothesis was intimately related to my field of interest, and that the Gibbsian form of the ergodic hypothesis that time averages in random systems were related to phase averages needed a new justification. This justification came from the work of Koopman and von Neumann, and particularly G. D. Birkhoff. Later on I was able to bring some of this work into closer relation with the theory of random processes than had been done by some of the other workers in the field. All this contributed to the growing technique of the field of work in which I had interested myself.

Here I must comment on the valuable discussions that I had at the time with Eberhardt Hopf who had come at that time from Germany to do some work at Harvard University. Hopf is an astronomer as well as a mathematician, and he turned my interest in the direction of certain integral equations which come up in the theory of the internal equilibrium of radiation. These integral equations, while they arose from an entirely different field of work, contributed significantly to the techniques which I was later to use in that field, and particularly in the theory of prediction. This theory of prediction, although it belongs to a later period of my work, is intimately associated with the prediction of weather, and therefore with the general statistical problem.

During the period of the late twenties and thirties, I received a very considerable amount of stimulus to and interest in engineering problems from my presence at the Massachusetts Institute of Technology, and was consulted more than once by my colleagues in connection with the task of reducing the unorthodox mathematical method of Heaviside to a rigorous basis. Here Vannevar Bush, in particular, pressed me to work in this direction and to collaborate with him in the writing of a book on electrical engineering operational methods. My interest in the operational calculus both received a great impetus from my studies of harmonic analysis and contributed to the broadening of my point of view in that work so that it forms an intrinsic part of the-intellectual machinery which I was later to use in the study of random problems.

Moreover, Vannevar Bush was intensely interested at the time in an instrument known as the differential analyzer by means of which he was able to give a numerical solution to many problems arising in the study of differential equations, and in particular in operational calculus. This started a long-time interest on my part in the possibilities of mechanical computation by means of analogue devices. I, myself, was led by this contact with Bush to the design of an optical method of obtaining Fourier transformers.

Bush's work was of the utmost importance in the study of the solutions of ordinary differential equations in which a single variable such as on time is the independent variable. Bush was very much interested in extending this sort of work to the solution of partial differential equations, and asked me explicitly. if I had any ideas as to how his instrument could be adapted to this purpose. It was clear to me at the beginning that the really difficult problem in the solution of partial differential equations was that of a manageable representation of functions to two or more variables so that they could become accessible to mechanical means of computation. I had already been much impressed with the incipient developments of television methods and with the idea of scanning which makes it possible to represent a function on two or more variables in the one variable of time. I suggested to Professor Bush that this technique of scanning should form an essential part of any mechanism for the study of partial differential equations. It was clear to me at the beginning that scanning methods involved an enormous compression of a great deal of information spread out in several variables into information in the one variable of time. I had already certain doubts as to the practicability of performing this compression by the use of analogue devices, and in my own mind I was already beginning to ask myself whether methods of a digital nature might not be more apropos. However, at this time the concept of digital computation was far in the future, and I did not go out of my way to implement these ideas by more practical speculations. Nevertheless, when just before the First World War the problems of the sort that Rush had raised had become of immediate practical importance, the idea of a digital computer and its relation to scanning was already prominent in my mind.

It was the Second World War, or rather the period of preparation j just before the Second World War, which led me to an abrupt turn in the directions of my interest. Before America had entered the war it had become amply clear that the possibility of this entrance was very actual, and that the immediate problem that faced us was that of keeping England from succumbing in the Battle of Britain. Here two directions of work came to the fore. One was the development of radar which had already been initiated by British scientists, and which caused the need of a greatly refined technique of electrical engineering, particularly in matters concerning alternating current theory and the related theory of communication by fluctuating currents, which had already proved so fruitful in telephony.

I had already interested myself in telephone theory and in the design of wave filters, particularly through contact with professor Y. W. Lee who had been a graduate student at the Massachusetts Institute of Technology in electrical engineering. Actually I came into contact with Lee because I had certain mathematical ideas on the use of Laguerre functions in the design of wave filters, and I needed a graduate student to work under me in this field. Lee contributed many essential ideas of the design of such filters. We proceeded to make the series of inventions which ultimately developed into patents and were sold to various firms, including the Bell Telephone Laboratories. When Lee had returned to China in the thirties, I received through him an invitation to lecture at Tsing Hua University at Peking. I accepted this invitation and we spent our time together there largely in the further elaboration of these inventions.

When the prospects of the United States entering the World War became imminent I tried to see if any of my own work would prove relevant to the military effort. I actually thought of the work that I had already done on the mechanization of partial differential equations. This was about the time of the Mathematical Society meeting at Dartmouth in the summer of 1940. At that time we were shown a computing instrument made by the Bell Laboratories for the study of a complex algebra which is used in electrical circuit work, and this made use of the Binary System of notation. Putting all the ideas together that were in my mind, I carne to the conclusion that my methods for the study of partial differential equations could be made practical, and that they would involve a digital computing machine rather than an analogy computing machine in order to obtain the speed that was necessary ; that this digital computing machine should be based on the scale of a rather than of 10 so that the individual marks needed for the different digits should be of a "yes" or "no" character, which is suitable to electronic circuits; and that in some cases this machine might work on the basis of previous ideas of H. P. Phillips and myself wheʳe we found the situation of the potential problem to depend on an infinite sequence of averagings.

This sequence of averagings could readily be adapted to a scanning process, but the thing that struck me as most difficult about this process was the vast body of intermediate computations which it would require, and the enormous mass of subsequently useless data which would have to be written down. However, it became clear to me that a really practical machine would have to perform all its functions on the run. It would have to write quickly, read quickly what it had written down, and erase material already employed as quickly as possible so as to have its entire functioning immediately ready for new data. I gave a series of prescriptions of what would be required for such a machine, and I passed them on to professor Bush who was then in charge of the national scientific effort. Bush thought that my work was too far in the future to be immediately applicable, so that for the time being I abandoned this direction of effort. However, the series of five requirements which I laid down for my computing machine have all been found to be valid, and are still the basis of such work as that which has been done on the International Business Machines.

When the radiation laboratory was set up at the Massachusetts Institute of Technology in view of the emergency, I participated in their work and sought to introduce my colleagues to the concepts of filtering which we had already developed. In addition, I saw several opportunities for the introduction of the study of random functions into engineering techniques. I went a certain distance in this direction, but with relatively small immediate success. In fact my time was already heavily taken up with the other problem which was critical at the moment - that of the mechanical control of anti-aircraft fire.

I had in view a method of prediction based on some purely abstract mathematical considerations. It turned out that we could simulate the experiments necessary to verify the validity of this mathematical prediction on Bush's differential analyzer. We did this, and we found out that the method would function, provided that the curve which we wished to predict was sufficiently smooth. However, when the curve lacked this smoothness it became clear that our prediction apparatus would be highly unstable, and that after any abrupt turn in the curve there would be a considerable period before the machine would settle into an equilibrium which would make exact prediction possible. Thus we found ourselves confronted by two difficulties which worked in the opposite direction. The very improvement of a predictor which would make it valuable for curves of a given smoothness was associated with an instability which would make the prediction very dependent on the smoothness of the curves to which we should apply it. It became apparent to me that this dual difficulty of prediction had a certain analogy to the principle of indeterminacy in quantity, and that it probably belonged to the very nature of prediction itself.

Since this difficulty could not be eliminated by any method, it became necessary to consider how the errors of prediction could best be reduced in practical problems. Here it became clear to me that the reduction of the errors of prediction in an optimal manner was dependent on the particular statistics of the curves which we wished to predict. I was thus thrown back to my old ideas of the statistical distribution of curves.

One of the prediction problems was conceived as a minimization problem which could be set up mathematically, and there was some hope of solving it. I found that the minimization problem in question led to an integral equation of a type closely akin to that which Eberhardt Hopf and I had previously discussed. I was able to solve this problem which led to a program of the design of antiaircraft predictors in which Dr. Julian Bigelow and I participated on behalf of the government. While this program did not lead to any single predictor design which was operated in practice, the ideas came to be applied to many other projects, and have carried over into the modern work on controlled missiles. The outcome of our work was a paper published for government use during the war, and reprinted after the war without restrictions, known as Extrapolation and Interpolation and Smoothing of Stationary Time Series with Engineering Applications.

This practical interest in computing machines led me to consider the general philosophy of the problem. On the one hand it became clear that the mechanism of a computation which depended on two value marks for the different digits could be easily adapted for the use of a machine to perform calculations of the algebra of logic, rather than numerical algebra. Here the two digital possibilities would correspond to the two possibilities of truth and falsity. Next we began to see that there was a certain analogy between digital computing machines and the human brain, particularly because of the fact that impulses in the nervous system seemed to be of an all or none nature, or in other words to involve two digital possibilities.

It must be borne in mind that our main work was the design of a computing machine to he used in the control of anti-aircraft fire and that we were not only concerned with the ways in which decisions were made but in the ways in which these could be realized in action. This led inevitably to speculations concerning the way in which the human being or the animal performs, purposive action. T his was a problem which arose in the consideration (a) of how the observer following the airplane is able to keep his sights on the plane, and (b) in the study of how we could simulate the action of such an observer under laboratory conditions. Here Ireceived suggestions from two quarters. On the one hand Mr. Bigelow took an active interest in the problem. On the other, through my friend and colleague, Professor Manuel Sandoval Vallarta, I had come some years before in contact with Dr. Arturo Rosenblueth who was then working on neurophysiology with Cannon at the Harvard Medical School, and who is now at the lnstituto Nacional de Cardilogia in Mexico. Dr. Rosenblueth ran for some years an evening dinner seminar on neurological problems at the Harvard Medical School and I had participated in this. It was therefore Dr. Rosenblueth to whom I turned with the physiological implications of my problem. My idea was this : in control apparatus one of the ways to stabilize the action consists of feeding back a quantity, depending on the success of the action, into the control apparatus as a new governing piece of information. Since any overshoot in this feedback is compensated by a corrective action in the opposite direction, such a feedback is known as negative. It had occurred to Bigelow and myself that such simple human actions as driving a car were governed by negative feedbacks. We do not move the steering wheel on a car according to a set pattern, but rather in such a way that if we find ourselves too far to the left we make a correction to the right, and vice versa. Therefore we were convinced that negative feedback plays a part in the human control mechanism, and in particular in the mechanism by which we follow an airplane with our sights.
This idea struck me as capable of verification or contradiction. It is well known that the feedback in a control apparatus must be limited if it is to have a stabilizing effect. Otherwise with an excessive feedback the apparatus goes into a spontaneous oscillation which becomes more and More intense, and ultimately either destroys the apparatus or at any rate brings it widely out of control. The question which I asked Dr. Rosenblueth was the following: are there any human pathological conditions on which the attempt to complete a voluntary action leads instead of its efficient performance to a wild oscillatory error ?

Dr. Rosenblueth answered me that such conditions were indeed well known, and that they constituted what is called purpose tremor or cerebellum tremor because they seem to be associated with malfunction of the cerebellum. A patient with cerebellum tremor when he reaches out his hand to pick up a glass of water will go into wild oscillations, and either will spill the glass or be incapable of grasping it. This fact confirmed my conjecture that purposive action may take place by feedback, and that cerebellum tremor is merely a case of the general process of breakdown of an overloaded feedback.

The period of the war was a very busy one for me. After the war I found that an abrupt change in my mode of work was necessary. I found that the pressure of military or quasi-military work was not for me, and it was borne in upon me that the moral hazard of working in a field primarily devoted to destruction, and in which I would be subjected to the vicissitudes of secrecy and of the lack of any share in determining the use to be made of my work, rendered further pursuit in this direction impossible. I decided to work further with Dr. Rosenblueth who had gone back to Mexico, and received support from the. Rockefeller Foundation for several years' joint effort with him. I was particularly interested in the study of clonus, and in general of the harmonic analysis of rhythmic physiological processes.

It was at about this time that I put together my ideas and those of several persons with whom I was in contact in the form of my book on Cybernetics. The book was bespoken by the late Mr. Freymann of Hermann et Co., and also received the support of the Technology Press and of John Wiley and Sons Inc. It represented a definite staternent of my thesis that communication and control theory belonged together, both in the machine and in the living organism, and that the basis for this theory was probabilistic. I had already seen my probabilistic ideas taken up with great definiteness by my colleague, Claude Shannon, then of the Bell Telephone Laboratories.

The thesis which I made in this book had implications for the sociology of the age of automatization. It had become clear to me that the human brain gave some sort of an index of what automatic machinery could do and was subjected to the same principles. I saw that the digital computing machine was primarily a logical rather than a numerical machine, and could be adapted to the control of factory processes. It was necessary for me to take a definite point of view with regard to the moral problems posed by this new industrial revolution which was clearly under way. It was in this connection that I wrote my book on << The Human Use of Human Beings >>

Let me note that the whole nexus of ideas which I then introduced had since passed from the stage of merely speculative possibilities into that of actuality. Furthermore, in the United States, in Russia, and elsewhere, the point of view which I then expressed, that the problem of automization was essentially a statistical problem involving the use of random functions of the Brownian motion type, has been taken up on both sides of the Iron Curtain.

Of late years my interest has become more and more devoted to the study of rhythmic processes in living organisms as generated by the response which is generally non-linear of such organisms to random inputs. Early in the thirties improved electrical techniques had made possible the precise study of electric potentials of nerve and muscle, and in particular certain fluctuating potentials generated by the brain and observable by means of electrodes placed on the scalp. This was the work on electroencephalography.

The early work in this field involved a maximum of experience and judgment in the reading of these brain waves and a minimum of mathematical techniques. I saw from the beginning that it was an appropriate field for the application of my ideas on generalized harmonic analysis, and in particular the use of the correlogram which I later saw to be the essential equivalent of the employment of the Michelson interferometer in optics. My colleagues at the Massachusetts Institute of Technology greatly furthered this work by the introduction of an appropriate instrumentation, and I pursued the field in collaboration with Dr. Walter Rosenblueth of the Massachusetts Institute of Technology and Dr. Mary Brazier of the Massachusetts General Hospital. In the course of this work it became clear that the brain waves had a fine structure which must escape the coarse meshes of the existing methods of analysis. I found that at the center of the alpha rhythm of about 10 cycles a second which seemed to be associated with visual processes there was a very narrow and sharp band of activity with a definite frequency pattern. In this pattern a sharp central line was associated with the depression of activity in its neighbourhood and represented a physiological index which promised to be of great theoretical and medical importance When I had found this pattern, it occurred to me that I had obtained a general result which belonged to the study of non-linear processes stimulated by a random Brownian input.

I pursued this work both on an experimental and theoretical phase, and in the summer of 1957 the late Professor Aurel Wintner of Johns Hopkins University wrote a paper on the subject. It became clear to me that in this response of non-linear systems of random inputs I had a clue as to how physiological processes could organize themselves into a definite synergic activity. I am at present engaged in pursuing my ideas along these lines.

The problem of organization which I had also developed in a paper which I gave at the University of Southampton in 1955 was a subject with profound sociological as well as biological significance and was connected with the theory of information by the most intimate bonds. I also continued my work in this field during my stay in India in 1955-56 at the Statistical Institute in Calcutta, and I saw how it could give a definite rationale to the concepts of social and economic planning which were much in vogue there.

My concept of economic planning is the following. In any economic situation there are certain factors beyond our control which are given statistically. These include the weather, the fertility of the crops; and other factors of the sort. In addition there are certain factors which we can control. For example, the amount of seed grain to be planted, the rate of interest on agricultural loans, etc. The problem of planning is to optimize, or in other words minimize, some quantity depending on the controllable and the uncontrollable statistical factors in such a way that this minimization is maintained on the average. Such a problem is of a statistical character, and therefore of an informational character.

From the economic point of view, this is what we may call the statistically stable planning problem. If we have any planning situation which is meant to continue, it must of necessity lead to a statistically stable planning situation. On the other hand, for a planriing situation to be statistically stable, it is not necessarily true that we can arrive at it from existing conditions in a statistically stable manner. In other words, this concept of statistically stable planning is only part of the social planning problem and must be supplemented by other conditions which enable us to make an effective planning of transient states and which arrive at a statistically stable situation.

It will be seen that cybernetics is leading me to a whole group of problems concerning organization, and it is in these fields that I think that an important part of the future of cybernetics lies. I have already mentioned the problem of self-organizing systems. The concept of self-organization is well known in biology, where there is a great deal of talk of material organizers of substances which in the embryo will cause different organs to come into being as for example in the case where a piece of an optic cup of a newt embryo inserted under the skin of the regenerating tail will cause an eyelid to form itself, and even possibly the rudiments of an organ of hearing. The aspect of self-organizing systems which has interested me most is that of systems which organize themselves into a rhythm. For example, in the formation of the vascular system of a vertebral embryo certain contractile cells form which very soon constitute a heart with a regular beat. How do these cells pull themselves into a concerted action?

The situation has occurred to me that these cells lend a double status as organs of information. On the one hand they give out electric impulses which can affect other similar cells. On the other hand, they receive such impulses and their action is modified by their reception. If the relations between these organs as senders and as receivers were linear, then they could not modify the frequency of oscillation to one another. If, however, there is a tendency for the frequencies of two vibrating members to interact either to pull one another together or possibly to push one another apart, there is a possibility of organization. Such a system as it gathers greater and greater synchronism will emit an impulse which has a greater and greater tendency to synchronize oscillators which have not already been pulled into place until by a mass action they constitute a definite pulsating organ. We have an example of this in electrical engineering systems where many alternators are connected to the same bus-bars. In this case the generators which tend to run fast or ahead of phase will carry a greater load than normal, and those running slow or behind phase will carry a smaller load. The result is to speed up the slow members and to slow down the fast ones. Even if for the individual members the speeding up and the slowing down is controlled by special governors for the individual generators, the whole system will contain a virtual governor more active than any of its component governors It is interesting to notice that this virtual governor is distributed over the whole system and cannot be located in any particular part of the system. This suggests that in many problems of organization, as in the case of the brain, we may have given way to an excessive tendency to suppose a sharp localization to function.

In the case of brain waves we have ample evidence of the existence of something of the nature of local oscillators. We also have ample evidence that these oscillations can act on one another in frequency. We know that the brain can be driven hy flicker which will tend to pull the rhythm of the brain into phase and frequency with itself. Under these conditions the sort of hypothesis which we have here made concerning self-organization systems is quite reasonable. It can also be pointed out that in the case of such self-organizing systems it will be quite common to find a sharp emerging frequency surrounded by regions of less activity than we find in the immediate neighbourhood. This phenomenon, as I have said, is actually verified in the case of brain waves.

It might be interesting to speculate on other rhythmic phenomenon like brain waves which may have a similar explanation of oscillating organs pulling one another into the same frequency and the same phase. It has been observed, and also contradicted, that fireflies in a tree tend to flash in unison. Here, too, we have to deal with periodic organisms which act both as senders and receivers of messages. The firefly tends to flash in a more or less periodic manner, and at the same time it is entirely reasonable to suppose that the visual reception to flashes from other fireflies will affect its rate of flashing. Under these conditions it is not much to hope that we have an example of a self organizing activity which readily lends itself to experimental and theoretical study as by suggesting that further work be done in this direction.

So much for self-organizing systems. There is another group of cybernetic problems which interests me very greatly and which concerns the measurement of causality. If we have two series of events in time, when we study each one separately there is a certain amount of information given concerning its future, but its past is determined. When the past of the two series is simultaneously determined, we shall receive more information concerning the future of each than if we were studying them separately. This additional information rnay be regarded as a measure of the effectiveness of one time series in causing another. Without going into details, here is the source of a really metrical theory of causality.

These are some of the directions of further work in cybernetics which interest me at present. To carry them out in practice I have been forced to develop my theory of random functions considerably further than I had in the past along lines related to the work of professor Friedrichs. There are certainly other directions of work in cybernetics which have great interest, and I do not wish to pretend that in giving these directions which have interested me personally I have any intention to dictate unnecessarily the future of developments in this field.

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