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Why RO theory is so difficult to understand?

by Harald Solaas

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Requisite Organization:

turn organizations completely around:

- bring a substantial increase in productivity
- a dramatic improvement in the quality of life

and do it:

- in a short time
- at minimal cost.

By Harald Solaas

As a theory of action, Requisite Organization certainly makes an ambitious claim. It tells how to turn organizations around completely to bring about a substantial increase in productivity and a dramatic improvement in the quality of life at work for its members. And all this can be done within a short time and at a minimal cost.

Requisite Organization theory is the result of more than five decades of work by Dr. Elliott Jaques. It is based on various scientific discoveries, some of which will be referred to below, and is a truly systemic theory that accounts for the integrated operation of the organizational structure, human resources systems and managerial processes. Jaques defined it as an art based on scientific principles, like medicine or engineering.

In spite of the bold claim above, the impact of RO theory on organizational practice and thinking has been notably limited. The number of practitioners throughout the world is indeed small relative to the total numbers of professionals in this activity. Jaques's work is absent from the curricula of many, maybe most, universities. No major publishing

house printed and distributed his books, of which limited quantities of copies have been printed, and which are often difficult to find at the social science, business and management sections of large bookstores.

This appears to be a great paradox. How can it be that our societies of today, so much concerned with the production of wealth, with the quality of life at work, and with social cohesion in general should ignore such a powerful instrument for the achievement of these ends? If it is true that action based on this theory would make not only our work organizations far more productive but also incomparably better places to work in, then its limited acceptance is not only an unexplainable fact but an actual tragedy. This is the concern that I wish to address in this paper.

Is the reason for this limited diffusion that the claim above is wrong, or at best a gross overstatement of RO theory's potential? After all, those of us who have been active in the organizational field for several decades have witnessed more than once the development of new organizational panaceas that started off by making

the most ambitious promises, only to disappear silently and without glory a few years later.

This is not, however, the case of RO theory. In the course of time there have been many organizational interventions based on this theory in many countries in the world. It was my good fortune to be involved in one of them, directed by Dr. Aldo Schlemenson, in which Jaques himself acted as a consultant. He came to Buenos Aires several times for this purpose between 1997 and 2000.

An aspect not often appreciated about RO-based practice is that it is extraordinarily economical.

The results obtained by these interventions seem to support the bold claim above. These have been recorded in various documents, among them the text of Jaques's presentation to the Society of Consulting Psychology in February 2002 under the name *The Psychological Foundations of Managerial Systems*. Also, *Social Power and the CEO* includes in chapter 17 an account of

the remarkable Commonwealth Industries case. These cases are clearly documented, and show dramatic turnarounds both in “hard” indicators such as profits, market share and quality standards, and in “soft” ones such as the advent of social peace in high-conflict situations. What is more, RO theory renders a full rigorous explanation of the processes through which these results were achieved. This is highly unusual, if not unique, in the organizational field.

Beyond this, those of us who have had a genuine experience in RO-based consulting know about the harmony and concord that good HR systems may bring, and how prodigiously cost-effective they are. I find it necessary to use the word “genuine” because, regrettably, there is consulting that passes as RO-based by only copying some of the theory’s peripheral aspects, that does not produce these quality results, and is therefore presented as “refutation” of RO theory’s effectiveness. I will return to this below, and will offer examples.

So why is it that we don’t see a lot more of RO theory? Over the years I tried several explanations. One of them is that RO theory runs counter to vested interests in organizational practice. An aspect not often appreciated about RO-based practice is that it is extraordinarily economical. If the management of compensations in industry, to make just one example, were based on RO theory, all the corporate compensation departments and the related consulting business would shrink to a tiny proportion of their current dimensions. It is not surprising that this may encounter resistance.

The time-span method is another prominent example. Quite apart from the problem of the validity of the measurements obtained, it usually takes no more than a few minutes of a single analyst’s work to measure a role using this method. Usual job evaluation methods, on the other hand, require a long and cumbersome process that requires the use of proprietary technology and involves several people.

Similar considerations could be made for the whole organizational consulting business. Once in a conversation with Jaques the subject came up of the large international consulting firms that disembark on a client company with a small army of juniors and charge fantastic fees. I remember him saying with fury in his voice, “I can’t wait for this to end.”

How good is this explanation for the resistance to RO theory? In my own mind, not very good. I do think it is true that it threatens vested interests, but I do not think this accounts for generalized lack of acceptance. After all, the search for effectiveness is always on and new things are being tried all the time. If this were the only reason, RO theory would have had its chance.

If more people, and the right people, became exposed to this theory, then its influence would increase.

An alternative explanation is that RO theory has not been sufficiently disseminated. If more people, and the right people, became exposed to this theory, then its influence would increase. Instead, the training of both

social scientists and corporate managers is based on concepts, traditional or faddish, that are not only alien but often actually opposed to RO theory.

Jaques himself attached great significance to the flawed educational background of people who design and run organizations. I remember he once said, “people worry so much about pollution in or present world. You know what the worst kind of pollution is? Symbolic pollution.” This point is strongly made in *Social Power and the CEO*. To him, current conceptions on organization, management and human resources were a symbolic malady which he saw as originating in the United States and unfortunately spreading to economically advancing nations in other regions of the world (page 13). When he spoke to audiences in Argentina he used to say that Argentine managers were in better conditions for change towards requisite organization than American ones, because they were not as badly influenced by misconceptions. And he meant it: he did not say this out of demagoguery, which was completely alien to his nature. This is really curious in a world in which the business communities look up to the US as the world Mecca of management theory. There is more than one way in which his thinking ran against convention.

Whereas this flawed background hypothesis is quite valid, in my mind it leaves a question unanswered. After all, as Jaques himself said, there was a time in the 1970’s when RO theory was taught in many universities throughout the United States. (The nickname “time-span Jaques” dates from this period). Also, many academics and high-level executives

have been exposed to it. Jaques published many books, and has been vastly quoted in literature by other authors. Why is it that RO theory has not made deeper inroads into organizational thinking?

RO theory is difficult to understand not because it is inherently difficult, but because it looks similar to other developments, and

The full answer to this question is uncertain, but I think I can identify an obstacle that stands in the way and that should be removed: *RO theory is almost universally misunderstood*. I have a conjecture on the reason for this, that I would like to submit to the opinion of colleagues. I believe that RO theory represents a radical paradigmatic shift within organizational thinking, with the unusual characteristic that this break is not obvious. In more simple terms, RO theory is difficult to understand not because it is inherently difficult, but because it looks similar to other developments, and it is not. Whenever we learn something new, we try to understand it by assimilation to previous knowledge on similar matters. Applied to RO theory, this only leads people astray. The problem in understanding RO theory is that it is so absolutely idiosyncratic.

RO theory differs from all other approaches to the study of organizations in that it is rooted in human psychology; however, the practice of RO is totally unlike what psychologists do, both in the clinical and in the organizational fields. Thus neither the organizational practitioner nor the psychologist can understand it

by assimilation to what they already know and do. However, both will try.

Human knowledge progresses in a discontinuous process, in which old paradigms are relegated in favor of new ones, and this discontinuity is generally obvious. When Copernicus devises a representation of the solar systems that puts the sun at the center, he has obviously broken up with previous geocentric conceptions. When Darwin speaks of the evolution of species, the very title leaves no doubt about his rupture with fixed species theories. When Freud postulates the unconscious, the divorce from consciousness psychology is self-evident. Or Einstein's break with Newtonian physics, and so on.

The person who attempts to learn these new theories knows from the outset that he or she is tackling with an unprecedented development, and that any previous references he or she may have must be subjected to careful scrutiny. Even the detractor knows that if he aims to defeat the new theory, he must do it in its own terms.

Not so with RO theory. To people who initiate its study, it looks like existing references from their previous learning and experience are valid for comprehending RO theory, which is thus not understood as inaugurating a paradigm. They are on strange territory, but they do not realize this. They look for familiar signs, and seem to find them. And they get hopelessly lost. Once and again I have seen this happen even to bright and intellectually honest people. This happened to me over and over, and if I could rectify many misunderstandings it was only due to the chance of having direct exchanges with Jaques, of which I offer some examples

below. I could never have done it just by reading his books, and in this respect I am sure I am far from being alone.

It is thus that those who honestly expect to expand their comprehension or improve their practice through the learning of RO theory often end up with nothing more than new labels for the old apparatus, and thus incur in pseudo-OR practice as mentioned above. The presumed detractors shoot their shells way off target, fail to produce potentially useful criticism and add to the general confusion.

Obviously, the successful diffusion of a valuable theory cannot depend on the availability of its author, even during his lifetime. Blocks to understanding must be faced and suppressed. I would like to think this paper could be useful in this respect.

I believe much of the confusion surrounding RO theory could be avoided if both those who are interested in it and its critics kept always in mind three essential postulates of the theory:

- It is based on objective knowledge and objective measurement.
- It postulates that people have subjective perceptions of several variables that pertain to the world of work in organizations. These intuitive perceptions are deeply set, universal, accurate and reliable. They are a part of the human condition.
- Trust-inducement, of , is at the core of the theory.

I will try to substantiate this point of view below. There is nothing in the concepts on RO theory that follow that has not been written or said by Jaques, with the possible exception of my own

misunderstandings. My only intention is to bring to the foreground some specific points that mark the rupture between RO theory and conventional thinking on organizations.

The measurement of wishes

Back in the 1950's Douglas McGregor wrote:

So long as the manager fails to question the validity of his personal assumptions, he is unlikely to avail himself of what is available in science. And much is there. The knowledge in the social sciences is not sparse, but frequently it contradicts personal experience and threatens some cherished illusions. The easy way out is rejection, since one can always find imperfections and inadequacies in scientific knowledge.

Control Is Selective Adaptation

An equally important reason for management's failure to make effective use of current social science knowledge has to do with a misconception concerning the nature of control in the field of human behavior. In engineering, control consists in adjustment to natural law. It does not mean making nature do our bidding. We do not, for example, dig channels in the expectation that water will flow uphill; we do not use kerosene to put out a fire. In designing an internal combustion engine we recognize and adjust to the fact that gases expand when heated; we do not attempt to make them behave otherwise. With respect to physical phenomena, control involves the selection of means which are appropriate to the nature of the phenomena with which we are concerned.

In the human field the situation is the same, but we often dig channels to make water flow uphill. Many of our attempts to control behavior, far from representing selective adaptations, are in direct violation of human nature. They consist in trying to make people behave as we wish without concern for natural law. Yet we can no more expect to achieve desired results

through inappropriate action in this

“We can no more expect to achieve desired results through inappropriate action in this (human) field than in engineering.” McGregor

field than in engineering.¹

It is a sad observation that, more than half a century later, the digging of channels for water to flow uphill continues frantically. It is encouraging, on the other hand, to note that the work of Jaques brings us much closer to the achievement of McGregor's aspiration. McGregor saw the problem very clearly but, despite his claims for the validity of social science in business, he lacked true scientific bases. This is Jaques's great contribution: he has laid the foundations of scientific knowledge that enable us to explain and predict human behavior in organizations.

It was clear to McGregor that management was not a science, but he advocated for the need to bring scientific knowledge into it. Jaques repeatedly said that RO theory was an art based on science and, as mentioned above, always drew the analogy to medicine and engineering. Both McGregor and Jaques aimed to root organizational theory and practice on human nature.

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A science of human behavior in organizations would belong to the

¹ McGregor, D., *The Human Side of Enterprise*, McGraw-Hill, 1960, pages 8-9.

social sciences, often called “soft” sciences as distinct from the “hard” natural sciences. As we all know, “hard” sciences are rigorous, they support prediction and control and provide an effective basis for useful technology. In today's world they carry the prestige of true science, whereas the “soft” sciences are often viewed as imprecise, lacking in rigor and unworthy of this distinction. Their capacity to sustain effective prediction and control is highly questionable.

There is, however, much to be said in favor of the “soft” sciences. They deal with the things that are important for us humans: mental health, peace, justice, self-actualization, the creation of wealth. It is legitimate that we should aspire to valid knowledge on these issues so laden with human meaning.

Many attempts have been made to bring “hard” methods into “soft” sciences. In psychology there is the rigorous methodology of experimental psychology and behaviorism's obsession with observable phenomena. In economics, the econometric school, the positivistic school sociology, and so on.

People in the “soft” sciences are often wary of these “hard” approaches, and with good reason. These attempts typically end up either in rigorous research on the trivial and devoid of human meaning, or in a spurious pretence of scientific objectivity with no other foundation than the use of numbers. So social scientists tend to defend “softness” as a guarantee of the preservation of the purpose of their disciplines.

Now about RO theory: is it “soft” or “hard”? There is a problem here: it resists such categorization. Jaques himself decried this classification as a

block to the construction of true science. To him, science was science, neither hard nor soft. RO theory challenges the soft-hard antinomy. Jaques's stated purpose has been to develop a science that would have the rigor of the natural sciences while preserving the human meaning of the social sciences. That is what RO theory is about. It is not possible to advance in its comprehension before one sheds the soft-hard frame of reference.

When Jaques wished to provoke an audience, he would ask, "is it possible to measure a wish?" to contend afterwards that this can be done as rigorously as the size or weight of a physical object can be measured. This sounds like a weird idea, but it is good to keep in mind that the advance of science is based on notions that were once wildly counterintuitive, such as the earth being round or revolving around the sun.

The notion that wishes can be measured is outrageous both to social scientists and to people at large. Don't we all know that human wishes are in their very essence elusive, immaterial, evanescent? Surely wishes can be comprehended, but not measured. To assume that we can measure them like we measure a brick or a stream of fluid means surely to negate its very nature. Jaques used to compare this refusal to the believed impossibility of measuring acceleration of physical bodies in the times of Galileo. Was it not obvious that a physical object has to stand still for it to be measured? Galileo did not think so, and this let him inaugurate the field of dynamics.

In the 1950's, while seeking a solution for a practical problem in an industrial company, Jaques discovers

the time-span of discretion. He has called this "the biggest discovery of my life"²

Just like the rest of living creatures, us humans are permanently engaged in goal-directed behavior. Working towards a goal implies necessarily the construction of a future by the organism in question. This construction occurs in the present. An essential part of it is a *targeted completion time* of the intended action.

This time can be measured in hours, days, years and so on, but it is a different kind of time from the time of clocks. It is the time of intention, a kind of time that exists only in the present and that is a *property* of goal-directed behavior, just like mass is a property of physical objects. Jaques permanently insisted that it was necessary to include these two kinds of time in order to construct a truly scientific theory of human behavior that would preserve human meaning.

When a person works towards a goal, whether it be quenching his thirst or revolutionizing society, it is possible to question this person about the intended completion time. This time of intention may seem vague, but it is not. The person may answer that he or she does not really have an intended time of completion, but a few simple questions (mostly bracketing upwards, like for instance asking whether he intends to quench his thirst two days later), will quickly reveal the actual time of intention.

This time of intention is independent from the intentions of the observer, and can be determined by independent observers trained in the

process. Thus it satisfies the conditions of true scientific observation on a par with the natural sciences.

Whenever Jaques spoke to a professional audience he made a big point that the measurement of the time of intention was an equal-ratio-length measurement.³ This is the type of measurement scale used in physics, characterized by starting from absolute zero (no negative numbers; at zero value the property measured disappears together with its measurement), and supporting full addition of its magnitudes. These measurement scales are fully homomorphic with the measured phenomenon, which Jaques saw as the ultimate form of objectivity in science. They differ in this respect with all numerical scales used in the human sciences, which are usually interval scales.⁴

Something curious happens with this concept. Even though Jaques insisted on it all the time, I never heard anybody else ever mention it or write about it. His presentations of this topic always elicited the same kind of "blank face" reaction as the one I will refer to below under *Jaques and the dismal science*. As for myself, I read about it and heard it from Jaques many times, but I never did anything about it. It just remained in my mind as a piece of unconnected and therefore meaningless knowledge. I believe the same thing happens to most people.

Until one day, while writing on the evaluation of potential capability, I thought I had to try to understand this issue that Jaques attached so much

³ See *The Form of Time*, page 129.

⁴ *The Form of Time* includes a full discussion of this theme in section 11, Quantification in the Human Sciences.

importance to. It took me a few days of hard mental work, juggling with examples of different kinds of measurement, but I finally grasped it.

I use time-span measurements all the time in my practice. At the beginning my own data surprised me. Here were hard data from reality, impervious to my own judgment and wishes, that were so remarkably consistent and which held such great explanatory power of organizational symptoms. This, so to speak, burst in my face. Over time I have become accustomed to this. It is a highly unusual experience for a psychologist to deal with this kind of “hard” measurement. I often tell my students that this makes me feel like an organizational engineer rather than a psychologist.⁵

Conceiving time-span measurement as an equal-ratio-length scale is of great help in understanding its objective nature. It dispels common misconceptions about this instrument. For instance:

- *Time-span is a “single factor evaluation system”.*

In reality, factors are conventional, and their evaluation is subjective. Time-span is factual, and its measurement is objective. Factors are *attributes* (and it is often not clear whether they are attributes of the role or of its incumbent), whereas the intended time of completion is a *property*.

- *Time-span is subjective, because it depends on managerial decisions, and these are changing and uncertain.*

In fact, however uncertain they may seem, managerial task assignments are objective facts,

measurable in terms of time of intention, that set the actual framework within which the subordinate works.

- *Time-span is “merely indicative”, and should be complemented by other observations, such as the type of problems the work involves and the mental processes required to do it.*

Mental processes are a property of people, not of work itself. We certainly want to avoid a throwback to trying to size up a role by the characteristics of its occupant. As for the types of problems, they do not automatically determine complexity. I confess I do not have a full understanding of this, but Jaques absolutely rejected trying to establish the size of a role by looking at the problems its incumbent had to handle. I remember him once offering the example of a general manager’s role that had all the appearance of parallel processing (stratum IV), with accountability for the coordinated functioning of several departments, that had been measured as stratum III. I asked him, “you mean the incumbent was a serial processor?” “Yes”, he replied.

We certainly want to avoid a throwback to trying to size up a role by the characteristics of its occupant.

This is not to say that time-span cannot be subjected to double-checks. I do that. After taking a measurement, I check for consistency with other variables. *But the purpose of this is to detect possible errors of measurement in order to decide whether to check on the longest task again, and not to complement time-span measurement.* The guarantee of true objectivity lies in time-span

measurement, and not in other types of data.

So, after all, wishes *can* be measured. I believe that the acceptance of this notion of the objective measurement of wishes can serve as an acid test of the comprehension of RO theory. Those who find it acceptable will have taken a big step in this direction. And those who do not think that the measurement of wishes is possible, or relevant, may attempt to invalidate a notion that is really at the heart of the theory, which would be a gigantic step forward from the nonsensical pseudo-criticism so often leveled at RO theory.

The following paragraph closes the section *Quantification in the Human Sciences*:⁶

By taking intentional goal-directed activity and episodes as the starting point for the construction of a scientific study of social and psychological phenomena, it is possible to put the human sciences on precisely the same footing with respect to measurement and quantification as the physical sciences. Paradoxically, the more “human,” the more full of meaning, intention, desire, need, will, feeling, we keep our psychological and social sciences, the more quantitative and “scientific” they can become, in the sense both of the rigor and of the elegance of measurement.

Jaques’s respect for objective data was the reason why some people mistakenly perceived him as dogmatic or authoritarian. On the contrary, he was infinitely patient and willing to listen to anybody, but he didn’t put up with the situation in which he said, “I have these facts” and people responded, “Oh, but I don’t agree.”

⁵ It is interesting to note that in *The Psychological Foundations of Managerial Systems* Jaques abandons the use of the term “human resources” in favor of “organizational engineering”.

⁶ *The Form of Time*, p. 195

Work strata are primarily about philogenia, not mental processing

Work strata are central to RO theory. There was a time when the name Stratified Systems Theory was used to designate it. So if one wishes to present this theory, one should explain the stratification of human work. The usual way to go about this is to explain work strata in terms of mental processes. I have heard this done by others many times. I used to do it myself, until I had an experience with Jaques which I later recounted in an e-mail to him:

In one of the meetings we had in Buenos Aires you once asked me point blank what a work stratum was, and I started to develop an explanation in terms of mental processing while you shook your head emphatically. In the end you clarified that work strata are natural managerial layers. Yet every time I have heard anybody explain work strata the explanation has been in terms of mental processes rather than in those of the original discovery.

I could find many more instances of this kind. I am perpetually amazed by the incredible nonsense that even bright people say and publish about RO theory. What I am getting at is that I am increasingly convinced that practically nobody understands RO theory, even among people illustrated in it and those who practice it. Theoretical points such as the above are not accessory but central. In their absence it is not possible to understand RO as a theory of prediction and control of human behavior. The way I understand it the concept of philogenia is completely central to the theory, yet most people who approach RO theory miss it clean. Furthermore, I think only a genius could grasp RO theory by reading *Requisite Organization*. Not even a bright academic or a committed CEO could do it. Is this a communications problem? Maybe you would like to comment.

This was in March 2001. Jaques replied:

Your latest e-mail is filled with insight and deep understanding. It was terribly funny to read--Kathryn and I both had a good laugh--but also, of course, felt very sad.

You have managed to state a very serious problem. I wish I knew how to overcome it. Part of the problem, I think, is that no one has been used to a seriously scientific approach to the study of behavior--based upon precisely constructed concepts, principles, hypotheses, and even a bit of true theory. All suggestions on how to overcome this problem will be welcomed!!!

In this informal paragraph Jaques characterizes the RO theory's paradigm, and implies that it is alien to people in the organizational field. Do people reject the new paradigm? My own reflection is that they do not realize there is one in the first place.

Work strata cannot be understood without reference to the concept of philogenia. This conclusion is as far from obvious as it is important. This is how I learned it. I wrote to Jaques:

You say that "Leadership processes must be judged in terms of how far they reinforce mutual trust." This seems to be a strong idea in your current thinking. Your reformulation of Kant's categorical imperative is clearly in this line. What is not very clear to me is what is the proof of trust inducement. Your phrase sounds to me like saying that penicillin must be judged in terms of its curing effects on patients, as different from a factual explanation of its germ-killing properties. One does not arrive to the adequacy of one-stratum difference between manager and subordinates because it proves to be trust inducing, but because people actually recognize this distance as optimal and this can be explained in terms of mental processing. Would you comment?

Jaques replied:

With regard to your second question about the value of mutual

trust, and how you recognize it, here are a few observations. Employees are strongly aware of the extent to which they can trust each other to help and not to harm each other in their working relationships. The experience is intensely vivid.

Employees are strongly aware of the extent to which they can trust each other to help and not to harm each other in their working relationships.

I believe you are wrong in saying that one does not arrive at the adequacy of one-stratum distance in terms of mutual trust. What happens is that managers and subordinates find they can work together better, therefore rely upon each other, and mutual mistrust is replaced by a morale enriching experience of mutual trust. I have observed these consequences arising with organizational change towards requisite conditions.

These two paragraphs marked a turning point in my comprehension of RO theory. I wish they could be just as enlightening to others. They are a good example of the kind of notion that only a genius could grasp by reading Jaques's books.

I mentioned above that two of the central postulates of RO theory were the reliance on objective measurements and the existence of innate perceptions relating to the world of work in common. This is a foremost example. The emergence of work strata is the result of objective measurements (time-span of discretion) and the intuitive recognition of the level of work in which a true manager may operate. This implies sound perceptions of potential capability and level of work, both one's own and one's manager's. I

will return to this point in below, in *Objective and Subjective Meet*.

There are two stages of discovery in the theoretical articulation of work strata: that of the 1950's and that of the 1980's. The first stage was the consequence of a previous discovery: the time-span of discretion. Field data obtained by means of this technique were combined with another type of data: the intuitive recognition of the "real manager" by people at work. While conducting research with other aims, an unexpected regularity emerged: certain boundaries in levels of work, at fixed values determined by the time-span technique, that had to be crossed for a subordinate to recognize his real manager. This mysterious pattern, like that formed by iron filings on a sheet of paper with a magnet underneath, replicated itself over and over irrespective of type of organization or national culture. This research is described in *A General Theory of Bureaucracy*. A schematic presentation of the results can be seen in *Requisite Organization*, page 40.

At this stage the universally recurring pattern of work stratification was repeatedly confirmed, but the reason for this phenomenon remained unknown. The revelation came in the mid 1980's, with the discovery of the almost perfect correlation between naturally occurring work strata and qualitatively different types of mental processing.

I would like to suggest that if one wishes to convey to others the meaning of work stratification, one should begin by explaining the first stage, and proceed to the second one only after it is clearly understood. In my own teaching I conclude the explanation of the first stage by remarking the following points:

- The stratification of human work is an unexpected discovery, and not an idea or a norm about how organizations should be structured.
- The existence of work strata is refutable in the sense of the hypothetical-deductive method. Any researcher could reproduce the conditions under which stratification manifests itself. As far as I know, this is a unique characteristic of RO theory, and one that sets it apart from all other conceptual developments in the organizational field. This is a mark of the new paradigm.
- Work strata relate to a profound reality of human work, one that transcends time and culture.
- Work stratification relates directly to trust, avoidance of mutual damage, preservation of individuality.

Work stratification relates directly to trust, avoidance of mutual damage, preservation of individuality.

The discovery of the correlation between work strata and mental processing opened up a whole new field of theoretical development, as expounded in *The Life and Behavior of Living Organisms*. It is a paradox, however, that the explanation of stratification that relays only or mostly on mental processes can act as a block to the comprehension of the true meaning and the origin of work strata. Instead, philogenia is central to stratification, as it is to the whole of RO theory.

Requisite organization is real, not ideal

How applicable is RO theory in real-life organizations? You have probably heard this question asked by

people interested in the theory. It is interesting to note that the sole fact that this question is asked reveals that the asker speaks from outside the RO theory paradigm. In fact, he or she is overlooking the meaning of the very name, *requisite*, "that which is required by the nature of things".

The usual approach to organizational improvement starts from an observation of dysfunctional phenomena and ends with an imaginative proposal of an organizational design that should overcome them. This might be called a "hopeful invention" paradigm.

RO theory follows a different path. It also starts from the observation of dysfunctional phenomena, but then wonders what is going awry in the actual organization with respect to that which is required by the nature of things. The fundamental notion is that organizations should be designed in a certain way because of the way us humans are. The articulation of requisite organization is the result of discovery, not of invention. This marks a radical schism with the "hopeful invention paradigm", whose logic runs something like this: "On the basis of such and such facts, I propose that the following change will make organizations work better".

RO theory is . . . a working tool to understand organizations as they actually are, including their pathology.

RO theory is normative, in the sense that it states how organizations *should* be designed. This is a feature it shares with many other developments

in the organizational field. There is, however, a fundamental difference: norms emerge from the results of research and genuine theory, not from the imagination of the theoretician.

One may well ask whether the matrix organization concept is applicable, or whether self-managed groups, or “skunk works”, or quality circles, or whatever, are applicable. These proposals represent somebody’s ideas on how organization might be designed to work better, and if one is interested in them, it seems reasonable to try them out in practice to see how they work.

In this vein, many people who approach RO theory tend to see it as an idealistic model that does not always correspond to organizational reality. Thus they raise questions about whether it “can be applied”. However, RO theory is not an ideal abstractly formulated by someone, but a working tool to understand organizations as they actually are, including their pathology.

The section above on the discovery of work strata is a case in point. Stratification in RO theory does not start as a proposal that a certain number of organizational layers is best (in the “hopeful invention” style). Rather, it enquires about the vertical work differentiation under which people can work best, in mutual support and confidence, and from this (in combination with time-span measurements) a natural discontinuity of work strata serendipitously emerges.

Failure to understand this originates much misguided criticism. If the critic is a detractor of RO theory, he will say that it is simply unrealistic and erroneous. If he is benevolent, he will say that it depicts a

worthwhile and desirable ideal, but one that doesn’t always reflect the harsh realities of earthly organizations. For both, the notion of Requisite Organization floats somewhere in a world of platonic perfection. Both are on the wrong track.

Let me draw an analogy. Modern medicine sets values that express the healthy function of an organism, such as a certain number of red cells per volume unit, minimum and maximum values of blood pressure, levels of cholesterol, ratio between tallness and weight, and so on and so on. These notions are so familiar to us that we may forget they were once startling discoveries. These indicators are of course the result of factual scientific findings. If we assume physical health as a value, then these indicators form a normative framework. Now, nobody would raise the question of whether this framework “is applicable”. It simply is, and allows to define physical health as full conformance to it.

Likewise with RO theory. It identifies and defines in the first place the structure and processes that enable certain types of organizations (accountability hierarchies) to function and produce value for their social milieu. What we call a requisitely organized company is one whose structure and processes conform completely to the normative framework of the theory, but RO theory explains the value-adding capacity of any and all accountability hierarchies, and therefore also their pathology.

No organization can be entirely non-requisite. *Requisiteness is measured in degrees of deviation from requisite organization.* Beyond a certain degree, the organization

becomes unable to do useful work and therefore collapses. McGregor said in the quotation above that in the human field “we often dig channels to make water flow uphill” In reality, of course, water does not flow uphill, and yet human organizations, however non-requisite they may be, do manage to create value for society that allows them to survive. This means that some channels, however imperfectly, actually do let water flow downhill.

Requisite organization theory accounts for the actual value-creation processes of real-life organizations, however non-requisite they may be. Requisiteness is measured in terms of degrees of deviation from requisite organization, rather than by its presence or absence. The “requirements of the nature of things” cannot be suppressed: they always have their way at least to a certain extent, or the organization would collapse.

An organization may have many “straw bosses”, but some managers have to be real (that is, truly accountable for the work of subordinates). Compensations may be arbitrary and unfair, but not to the extent of total divorce from level of work and effectiveness. People can be made to work below their capability and given tasks that fail to elicit their commitment, but not to the point where they will be so alienated that they will just give up no matter what. Deviations from requisite organization bring organizational collapse if they surpass a certain limit.

In *The Psychological Foundations of Managerial Systems* Jaques illustrated this by comparing non-requisiteness to back pain. People who feel back pain can still get on with their work, though not as well as they

might have done. Maintaining the analogy, requisiteness would be the normal functioning of a healthy organism, not a normative pronouncement.

It is interesting to note that under extreme conditions the margins for deviation seem to narrow down. There is one situation in which requisite organization strongly tends to actualize itself. This is the case of the armed forces in combat. The effective army of a large country at war shows the following characteristics:

- Layers of accountability (and therefore authority of command) are defined by units of command, not rank. Seven layers exist from commander in chief to private soldier, no more and no less.
- Officers are held accountable for the results of the units under their command, not their men.
- Officers are held accountable to do their best to provide for the safety and well being of their men before they take care of their own.
- Everyone is held accountable for doing his or her best; no less is accepted. "Motivation" is taken for granted.
- Recognition is given by principals on the basis of overall effectiveness.
- "Incentive plans" for "increasing motivation" would be simply unthinkable as patently immoral and destructive of morale.

When the bullets are flying and life is at stake the one form of organization that leads to maximum effectiveness and care of its members strongly tends to emerge. In everyday civilian life, however (and in the military in times of peace), people who decide on the structure and systems of organizations are free to act with little legal or moral constraints. After all, all that is at stake is creation of wealth and the

well-being of the people. This is bound to change some day.

The principle of accountability

As everyone knows, the principle of managerial accountability plays a central role in RO theory. I have observed that some RO practitioners, acknowledging this fact, speak of introducing managerial accountability into an organization as if they were bringing in an inaugural concept and practice. The implication seems to be that this principle is hitherto absent from that particular organization. I believe this is an effect of being situated in the "hopeful invention" paradigm, and that lack of clarity by the practitioner on this point can only confuse the client and hinder the intervention.

In fact, managerial accountability is already there: it simply cannot be completely absent. Deficient or unclear managerial accountability is organizational pathology; total lack of managerial accountability would mean total pathology, which would lead to instant death. It is "required by the nature of things", for instance, that if an employee performs persistently below standards somebody somewhere in the organization *has* to take action at the risk having his or her performance evaluated as not acceptable (this is the essence of the "real manager" concept). This is true whether the accountability for the employee's performance has been explicitly assigned or not. The problem is the blurring of managerial accountability, not the absence of it.

This is what extant organization analysis is about: the actual structure of managerial accountability. A definition is:

Extant organization: the system as it actually functions, as demonstrated by systematic study. It will always be an approximate picture. It requires that you dig in and find who is actually being held accountable for what, and what authority they are in fact able to exercise in relation to whom and what.⁷

Any attempt to improve the system of managerial accountability should take the comprehension of the actual system as the starting point, and not try to teach managers about accountability for subordinates ignoring present reality. Jaques puts this in one phrase:

A slogan could be: **Discover the extant and use it as a stepping stone to the requisite.**⁸

The concept of managerial accountability was of course not created by Jaques. It is certainly known and used in the field by many authors. However, I know of no other account of the functioning of organizations that gives it such a central role. I believe that this simple, clear-cut concept is in itself a major contribution to the organizational field. The sole task of getting both managers and professionals to think about organizations in these terms can save enormous amount of time, resources and grief. It could act as a vaccine. Suppose one could get this concept straight across to some people, and then these people had to evaluate theories and experiences on matrix organizations, self-managed groups, quality circles, double-loop learning, mental models, empowerment, piece-rate incentives,

⁷ Requisite Organization, 1996, p. 33

⁸ Requisite Organization, 1996, p. 33

production bonuses, leadership development, and so on and on...

RO theory as a diagnostic tool

Another negative consequence of considering RO an ideal model is that it obliterates its value as a diagnostic tool. Jaques put great emphasis on the need for true diagnostic in organizational practice.

What is usually called “diagnostic” in this field is often no more than a compilation of complaints and aspirations from members of the organization, carried out in the expectation that the results will indicate what is wrong and what should be done to correct it. They do not.

“Poor communications” is a case in point. People show their dissatisfaction by expressing a complaint about communications, and the intended corrective action is to improve communications by such means as bulletin boards, newsletters, meetings with executives, mailings to employees and so on and on. In fact, the symptom people express is the effect of causes that need diagnosing, and which are not corrected by improving formal channels.

The use of employee surveys is another fine example. These surveys can be useful instruments for measuring the effect of organizational changes, but they do not indicate what the real problem is, nor how to solve it. The problem is that true diagnosis requires true theory. Jaques makes this point beautifully:

Comparison with the meaning of diagnosis in medicine can be revealing. Medical diagnosis comprises three components: *symptoms, signs and theory*. *Symptoms* are the patient's description of what is bothering him or her – the

headaches, or dizziness, or abdominal pains, sweats, or whatever.

But treatment based upon symptoms alone – symptomatic treatment – can never get at the roots of disease. It is essential that the physician should examine the patient to ascertain the significant *signs* of illness. This examination calls for the use of technical diagnostic procedures – not just the stethoscope and thermometer, but also the more sophisticated X-rays, complex blood tests, MRIs, etc. etc.

Once the signs are established, the physician must have recourse to a *theory* in order to evaluate the signs and symptoms to make a diagnosis. It is because of different theories that two physicians may arrive at two different diagnoses using the same diagnostic data.

This same process must be used for analysing managerial problems. Symptoms are readily obtained by interviews, discussions, and surveys with managers and with non-managerial personnel. The complaints will be familiar – poor communications, weak leadership, too many layers, no career development opportunities, nepotism, unfair compensation, back-stabbing, bureaucracy, too much consensus, too little consensus, lack of lateral cooperation.⁹

Further on in the same paper, he says:

The theory to be used here – namely, *Requisite Organization* – is different. It assumes that such behaviors are symptoms arising from faults in the managerial organization structure and processes.¹⁰

The usual pseudo-diagnostic process of collecting symptoms

mentioned above can be endlessly long and cumbersome, as it is not really looking for anything specific. RO-based diagnosis, on the contrary, looks for specific data with pinpoint accuracy. This another instance of the economy of RO practice. I have found in my practice that this can at first be bewildering for clients. Their reaction can be “Is this all?” “Can we really solve such an important problem with so little information?”

I would like to offer two sample cases from my recent practice.

The manager of a service department in a large company complains that one of his seconds in command tends to follow her own way, does not keep him regularly informed and in some cases ignores policies he has set. He insists on her “personality problems” that cause trouble. Time-span analysis (done with the purpose of evaluating potential of all employees in the department) reveals that the manager is working at stratum IV high, and his two direct subordinates in stratum IV low/medium.

A small business run by two partners (A and B), each of which supervises a group of operators. The symptom is that whereas partner A leaves ample room for the discretion of his operators, partner B attempts to instruct his own in full detail. B's operators feel constrained and dissatisfied. A few questions quickly reveal that the longest management tasks in this business are just under a year, and that these are done by A alone. B “doesn't like administration”, and prefers to leave them to A. A diagnostic hypothesis emerges: B's personal time horizon puts him in stratum I, so he is working in a

⁹ Jaques, Elliott, *Diagnosis of Organization Problems and the Achievement of Change*, 2000, p. 1

¹⁰ Jaques, Elliott, *Diagnosis of Organization Problems and the Achievement of Change*, 2000, p. 2

situation of compression with his subordinates.

In the first case, the optimal solution would be lateral expansion (more roles) at stratum III reporting directly to the department manager. By the way, this notably enlarges the availability of potential candidates. The temporary solution consists of “organizing compression”, as Jaques used to say. Compare this to the solution of coaching the subordinate who originates the complaints in order to help her “smooth out her personality problems.”

In the second case, the solution starts with an explicit acknowledgement of the differential level of work of the two partners. Predictably, maintaining the current situation would continue to build up pressure. The solution would involve a reformulation of the partnership itself. Compare this to the solution of indoctrinating partner B on the virtues of empowerment.

The observation of philogenia

Once the consulting team to which I belonged was conducting an evaluation of potential exercise at the higher levels of a large organization. This was in the context of the project I mentioned above, in which Jaques acted as a consultant. The managers who participated in the exercise were working with deep concentration and in complete harmony. At one point, Jaques called us aside and told us, “You are now watching ordinary people doing extraordinary things”.

This episode has remained as a vivid memory for me, and I think the main reason it has is that I would have missed the observation had it not been for Jaques’s remark. Philogenia was in full display right in front of my eyes,

but I wouldn’t have noticed it if I hadn’t had my attention pointed to it.

Observation is construction

The point I want to make is that if one wishes to validate the prediction that good organizational systems are philogenic, one should define the expected effects in advance and deliberately observe whether they materialize or not. Philogenic behavior is not an obvious phenomenon that just blows up in your face. This is of course a general principle of scientific observation: one should define the initial conditions and the effects these conditions will produce according to one’s hypothesis.

Lack of respect of this principle is the source of a form of pseudo-criticism of RO theory that is so very common that I am sure practically all practitioners of the theory have witnessed it more than once. Following RO theory, one states that given certain conditions, people will react in certain ways. For instance:

- Given a requisite personal effectiveness evaluation system, managers will evaluate their subordinates fairly (and not influenced by favoritism, personal enmities or demagoguery).
- Given an equitable pay system, people will be satisfied and will not strive for additional financial gain.
- Given a requisite job evaluation system, managers will not incur in cheating and politicking in order to influence evaluations.

“critics” contend that these predictions are mistaken . . . blind to the fact that their data come from *observation done under non-requisite*

And so on.

I am sure we have all lived the situation in which “critics” contend that these predictions are mistaken because their own experience contradicts them, blind to the fact that their data come from *observation done under non-requisite conditions*. I have found that this obstacle, obvious as it is, is very hard to beat. People can be enormously reluctant to accept that the predictions of RO theory must be tested under the precise conditions the theory defines, and that therefore their own observations are valid only to the extent that these conditions were present (and, of course, they usually are not).

Notoriously, conventional organizational theory does not fill the requisite of defining precisely the conditions under which a prediction would materialize. It is therefore legitimate to use one’s observation and experience to criticize any theory in point. But RO theory does define such conditions, and therefore this type of criticism is not valid. Once again, presumed critics are stuck in an alien paradigm and they do not even realize this.

The observation of paranoiagenia

I believe a good way to understand the nature and meaning of trust-inducing organization is by way of comparison with paranoiagenic forms of organization. I think we are all experienced in organizational paranoia-inducement.

From the standpoint of RO theory, our working organizations of today are saturated with paranoiagenic systems. In the following paragraphs, grim and colorful at the same time, Jaques expresses his feeling on current employment organizations:

... this role system now dominates our free enterprise democratic societies. Ninety percent of those who work for a living do so for a wage or salary in a role in this type of system. The systems have an enormous impact on everyone: everyday experience of trust or suspicions in relationships, and of economic security or anxiety; opportunity, or not, to exercise our full capabilities; career development or stagnation; differential economic status of families across the land; industrial calm or unrest. How we design these role systems in and of themselves, has become a matter of central importance for the health of society.

Not only does the managerial employment hierarchy dominate our free enterprise democratic societies, they are by-and-large social slums and swamps, with an overall socially disruptive effect on a scale that threatens healthy democracy. Employees from senior executives to the shop and office floor are packed like sardines in tins, squashed on top of each other in far too many layers; managers breathe down their subordinates' necks, with little to choose between them in capability, so that effective managerial leadership is a scarce commodity; full employment, crucial in social health, has never been established as a political right; compensation systems are a right royal mess; the chances of having work in line with one's potential capability are slight and spasmodic, and career and talent pool development systems are at best primitive; and on and on, the list of ills is endless. And our organizational gurus are adding to the mess with a seemingly limitless flow of fads, all of which make things worse.¹¹

Today we live immersed in paranoiac systems like fish live in the water. We do not even recognize

their existence. And they are often blatantly praised. Some time ago I read an article by Jack Welch in which he described (and defended) the performance appraisal system he had used in GE. Managers had to rank their subordinates according to their evaluation of their performance. Those at the bottom of the list would be let go (I have seen this in other companies too). He said the first year the system worked reasonably well, on the second more resistance was encountered, and by the third it was open warfare. Managers went to such tricks as placing at the bottom of the list people who were about to retire, or even employees who had died. Welch claimed that this was necessary in order to preserve the organization's effectiveness. This is no less than an open defense of paranoiacism.

We have an ample historical record of educated voices of the past which in all seriousness advocated for the legitimacy of slavery, the "scientific" inferiority of women or of ethnic minorities, the moral debasement of the poor, the inborn nature of criminal behavior, and so on and on. Torture as a legitimate judicial form of punishment was finally abolished only in the XIX century. It is worth noticing that all these notions were considered in their time not a problem, but a natural state of affairs. I believe that many human relations practices such as the above, which today we consider normal, will join this illustrious group sometime in the future.

Compare this appraisal system to that proposed by RO theory. Managers are instructed to evaluate their subordinates by classifying them in a six-band scale according to their judgment on their overall

effectiveness in discharging the accountabilities of their roles (and not against the performance of their peers). The bottom band represents the minimum effectiveness required to be acceptable for the role, and the top one effective autonomous handling of the most difficult problems inherent in the role. There are no quotas for the bands. If a manager, through his or her own good exercise of managerial leadership develops a team of "stars", those who would be ranked at the bottom will be classified in the upper band, and receive due recognition for their high-quality work. This is perceived as fair and equitable by everyone.

I participated once in a project in an organization that was in the process of change toward requisite conditions. This organization had a performance appraisal system which imposed rigid quotas for the different evaluation levels, which is, by the way, a very common practice. When a new system that would let managers evaluate their subordinates without restrictions was announced, many complained that it would only open the gates for the demagoguery and favoritism of managers, and that they would tend to put all their people at the top.

This is an example of the all-common mistaken predictions mentioned above. They are mistaken because they come from experience gathered under non-requisite conditions. What actually happened in the case above was, of course, that managers evaluated their subordinates fairly. They were also instructed to inform each employee of his or her evaluation, and a survey conducted after the exercise revealed that this was highly appreciated by them.

¹¹ Jaques, Elliott, *Reply to Dr. Gilles Amado*, in *Human Relations*, Volume 48, Number 4, April 1995, page 360.

The effectiveness evaluation system proposed by RO theory is a prominent instance of its reliance on the validity of subjective judgment when done under requisite conditions.

Where objective and subjective meet

For some years now I have been deriving great satisfaction from evaluation of potential projects in the context of talent pool development. The method I used is the one I learned in the project mentioned above in which Jaques acted as a consultant. This method is not identical with the one outlined in *Requisite Organization*, so I would like to present it briefly.

Samples of roles representing all levels of work are selected for all organizational units comprised. These roles are measured by the time-span method, and entered in charts. These charts show graphically all work strata in the organization, with three bands (high-medium-low) within every stratum. The measured roles act as landmarks that make the scale easily comprehensible to evaluators. In a large organization, several of these charts may be developed, so that all evaluators (managers and managers-once-removed) will find roles that are well known to them.

Using these charts, an HR analyst holds individual interviews with managers and MoRs and records their evaluation of every member of the evaluated population in terms of strata and of levels (high-medium-low) within the strata. Then a gearing meeting is held, with the participation of the MoR and his subordinate managers. The whole evaluated population is examined jointly, and any discrepancies are sorted out. The

result of the meeting is a final evaluation of current potential capability of every member of the selected population.

As mentioned above in *On the observation of philogenia*, this process normally takes place in a climate of acceptance and harmony. Why is this? Why is the evaluation of potential so successful and well accepted? I think the answer to these questions is all-important, and I would like to, as it were, put it in a frame:

Because subjective judgments and measuring instruments refer both to the same factual order.

Let me recapitulate what I mentioned at the beginning as two of the essential postulates of the theory:

- It is based on objective knowledge and objective measurement.
- It postulates that people have subjective perceptions of several variables that pertain to the world of work in organizations. These intuitive perceptions are deeply set, universal, accurate and reliable. They are a part of the human condition.

The evaluation of potential method is a fine illustration of this. On one hand, the scale used for evaluation of potential is objective in the strictest sense. On the other hand, it makes use of the subjective perception of other people's potential capability relative to the also subjective perception of the differential levels of work. Speaking about the human ability to intuitively judge one's own and other's potential capability, Jaques once said, "It's genetic".

Accurate perceptions of other people's potential capability and of differential levels of work are necessary requirements for successfully carrying out the demands

of daily social and work life. They are a part of the endowment that enables us to interact in society. And these innate abilities are used for the evaluation of potential. Jaques put this in a felicitous phrase: "the problem of the evaluation of potential is not a problem of measurement, but a problem of code". That is, we have the intuitive perception but we are unable to attach a shareable value to it. This is what the objective scale based on differential level of work does, and solves the problem.

Is this procedure objective or subjective? It is both. Objective and subjective meet. "Gut feeling" about differential levels of work of several roles and the objective measurements taken match. I feel gratified when I hear my clients say about the results of an evaluation of potential exercise: "One feels in the stomach that this makes sense".

I have found that this virtue of both instrument and intuition referring to the same factual order is not always understood among practitioners, not did I understand it myself at the beginning. Years ago I had direct experience with the evaluation of potential system at a large multinational European company. I was a human resources officer there at the time. This system used as a reference for the evaluation of individual potential the structure of job categories defined by the Hay method. Later on, while doing research for a book on the evaluation of potential, I found other companies, also large and European, that used systems of the same type.

One could think that both systems are essentially the same (I did at first). They both use level of work as a reference, and managerial judgment as

a source of information on individual potential of employees. However, the resemblance is only superficial, and they are in fact radically different. The RO system is based on factual references that meet intuitive perceptions, whereas these other systems rely on conventions and fail to provide a true measure of work level. Right from the outset evaluators feel that the references they are given do not match their intuitive perception of differential levels of work. Thus the system is felt as arbitrary and incites cheating.

I have observed some examples of inadequate RO practice resulting from this confusion between two different types of systems. One is that of colleagues who lightly encourage clients to use their current structure (manifest organization) as reference for the evaluation of potential, instead of carrying out the required extant analysis. Thus the level-of-work references are equivocal, and the credibility of the method suffers. Also, some think this method can be freely combined with other approaches, such as psychological testing or analysis of competencies, that do not share this intuitive soundness. These are examples of the unfortunate use of RO theory for attaching new labels to the old apparatus.

Jaques and the dismal science

Prevailing economic theory exerts a strong influence on many aspects of society, work organizations amongst them. Policies and procedures for the compensation of employees are possibly the most directly affected.

Two underlying assumptions impinge directly on compensation systems: that personal profit is the basic driving factor of all economic

activity, and that human labor is a commodity subjected to the fluctuations of supply and demand. It is quite easy to observe in practice multiple examples of how strongly these assumptions influence the way people in employment are paid for their efforts. To many people this approach to the problem of compensation in employment organizations is so obvious that they cannot even conceive of a different one.

In the 1950's Jaques introduces the concept of felt-fair pay. This is a discovery made possible by a previous one: the time-span of discretion.

Succinctly, the discovery consists of the following. In a research situation (that is, confidentially and for statistical purposes only), persons who work in employment organizations are asked what they judge as a fair and equitable compensation for the work they are doing at the time. They state a figure in monetary units. In possession of the time-span measurement of the role those people are working in, both types of data are analyzed jointly, and the following significant results emerge:

The level of pay stated as fair and equitable by the subjects:

- Does not correlate with the type of occupation they work in,
- Nor with their actual pay level,
- Nor with the market value of their positions,
- *But it does correlate significantly with the level of work as measured by time-span.*

Some mighty conclusions emerge from these results. Among them:

- It is possible to design a compensation system that will satisfy everyone.
- People are not necessarily driven by personal greed. They just play

this game when they have to, just like they can play the game of fairness and concern.

- Ordinary people hold a great potential for fairness in sharing with others, including the intimate acceptance that other people should make more money than they do.

In prevailing economic theory, the value of labor is set by supply and demand just like with any kind of commodity. There is, however, a big difference: items of merchandise are in no way affected by the market value of other items. People, on the contrary, are extremely sensitive to what their peers, principals and subordinates get for the work they do, and this affects deeply their well being, their commitment to their tasks and the level of interpersonal strife at work. Jaques used to say as an example that vinegar does not care whether the price of olive oil goes up or down, nor sand of bricks, but humans do care about how others are rewarded relative to themselves, and strongly. And they react to it. There seems to be no place in current economical science for this fact.

A funny thing occurs with this notion. I heard Jaques explain felt-fair-pay many times to different audiences. The reaction was always the same: faces of people went blank. Neither interested, nor hostile, nor even disagreeing. Just blank. Jaques said he made this discovery decades ago, and that it was met with "a thunderous silence" (I forgot where I read this; would welcome help).

This happened to me too. I had of course read and heard about felt-fair-pay, but it just remained there as inert knowledge. Until one day Jaques told me: "Nobody asks me about felt-fair

pay, nobody wants seminars on this". Then he pointed a finger at me: "You haven't asked me". He saw my baffled expression and told me: "Think about it".

Why this blockage to the comprehension of this notion, at once so simple and so far-reaching? To be sure, intrinsic difficulty is not the cause. Is it that it challenges a paradigm of wealth distribution in society that is deeply embedded in our Western culture? I find no other explanation at present.

The following quotation is from an e-mail Jaques wrote in August 2002:

Social mechanisms among humans reach very high levels of complexity in relation to economic trade and wealth distribution. I believe that economics remains in a very primitive state.

a) Easily observable is the fact that economists fail miserably to understand that human labor in employment organizations does not behave like a commodity on which we place values in accord with supply and demand. They are totally unable to consider any such possibility as the existence (factual) of universal felt-fair differential pay norms in employment systems.

b) I am currently reading up on economics, political science, and law, and have come to realize that these fields are applied arts like engineering, management, and clinical medicine. They have wasted all their time trying to formulate theories, for the underlying theories will be found only in behavioral science, just like the underlying themes for engineering are to be found not in "engineering theory", but in physics and chemistry. I am finding it an interesting experience to try to sort out what might be some of the elements of behavioral science that will be necessary to understand exchange and trading behavior, wealth distribution, valuation of goods, taxation, etc. etc.

Many notable economists, especially those most concerned with the welfare of world population, have severely questioned the assumptions of personal greed as the sole driving force of economic activity and of human labor as a commodity. They see these notions as barriers in the task of combating misery and exploitation in the world. And some are trying to build an economic science with a human face. As the quotation above shows, Jaques was working on this too. Just like organizational theory should be based on psychology (this is what RO theory is about), so should economics be based on behavioral science. It is very regrettable that this work in progress has not reached us. Maybe the dismal science doesn't really need to be so dismal after all.

RO theory, freedom and authoritarianism

RO theory is considered by some to be an outdated authoritarian model. It has been labeled "neo-tayloristic", even "neo-fascist". Jaques has been called "omnipotent" and "big brother". Supposedly it aims to rigidly specify everything in order to control human behavior. This is an extreme of misunderstanding.

As we all know, the concept of discretion is at the heart of the theory. Human work is defined as the continuous use of discretion in pursuing a goal.

As defined by RO theory, the mere compliance with rules is not work at all. The effort involved in work is in the use of discretion, not in following instructions. The felt weight of responsibility is a function of the required tolerance to ambiguity, and there is no ambiguity on whether one

is breaking the rules or not. The value creation of any worker is the result of the use of his discretion, not of abiding by the rules. Work itself is not only unspecified, but inherently unspecifiable. According to Jaques, the exercise of discretion is an ineffable process in which not only humans but all living creatures are permanently engaged. In this respect, us humans and all forms of simpler life down to the ameba are identical. Discretion is the ultimate form of subjectivity.

The following statements focus on the problem of freedom and constraints at work. All of them have been explicitly stated in numerous texts. It cannot be seriously questioned that RO theory postulates them.

- **All work is inherently creative. All work involves the use of discretion and is potentially a source of self-achievement for the person doing it.**

The problems in this respect (routine work, no room for creativity, lack of personal fulfillment) arise from a mismatch between level of work and current potential capability of the worker, and not from the design of the tasks themselves, as "job enrichment" and "empowerment" would have it.

- **With rare pathological exceptions, all men and women can be satisfied and productive workers. People can be successful by freely being their own selves.**

Jaques's first formula for Current Applied Capability included the (-T) factor, meaning that temperamental factors could affect effectiveness only in a limited number of cases, and then only in a negative way. No temperamental traits were required for good performance. It is interesting that in his most recent formulation (-T) was

replaced by (RB): required behaviors, defined as “ability to carry out the behaviors required by the basic established values of society”. If a person has a tendency to disruptive behaviors, it is for him or her to leave them at home or keep them under control. Apart from this, he or she is free to be his or her own self.

- **By the same token all men and women are potential good leaders. Specific behaviors and personality traits are irrelevant.**

The leadership phenomenon is clearly defined within RO theory as the systemic effect of a set of organizational and personal factors. The only personal trait required is an inclination to work with other people and to help others work better, which is abundant among the population.

- **The ordinary average human being holds a tremendous potential for equity, for collaborative work and for mutual trust.**

It only takes the right organizational conditions to unleash this potential. A unique contribution of RO theory is that it offers direct empirical evidence to support this statement: the results of research on felt-fair-pay.

To sum up: RO theory is profoundly optimistic about human nature. It assumes that the ordinary human being holds an enormous potential for equity, for honesty, for cooperation and for constructive work. It is a theory of mental health, different from those us psychologists learn: it does not attempt to change people, but to bring out their best through the requisite design of social systems. It is a theory about the generation of mutual trust among people at work. It is also a theory of human freedom. Jaques develops an elaborate discussion on constructive constraints as the foundation of

RO theory is the only theory in the organizational field that carries the scientific hallmark of refutability: and , until now, has not suffered any serious refutation.

Jaques says, “I am not saying my theory is good; what I do say is that it is the only one there is.”

freedom, especially in his latest writings, such as *The Life and Behavior of Living Organisms*. And this is what RO theory is about: the elucidation of the constructive constraints that are required by the nature of things.

One thing is starkly clear: the accusations of omnipotent control of human behavior can only rest on a flawed comprehension of RO theory.

Postscript

As any scientific theory, RO theory is refutable, and may one day be proved wrong or be advantageously replaced by a better one. In fact, as far as I know it is the only theory in the organizational field that carries the scientific hallmark of true refutability. I have heard Jaques say, “I am not saying my theory is good; what I do say is that it is the only one there is”. I have found that this phrase sounds boastful to many people, but this is only a consequence of unawareness of the dearth of true theory in the field. The intention was certainly not boastful, and at any rate that would not excuse the need for a serious

response. The assertion that it is the only theory may, and should, be challenged; however, one essential condition must be satisfied: that the challenger understands what it is about.

From the reading of this paper, it will be obvious to you that I strongly adhere to RO theory. However, my primary intention has not been to uphold its truth or its virtues, but to demonstrate its singularity. The refutation of a grand idea is grand in itself, and I for one would be filled with admiration for any thinker who would seriously put RO theory in question. Until this happens, it is fair to expect both from practitioners in the organizational field and from people in executive positions that they direct their attention to the ambitious claims stated at the opening of this paper. The feasibility of these desirable changes is well founded, and RO theory has not suffered any serious refutation until now. As things stand, it is the best way we have to bring about a significant impact in society by improving the creation of wealth and the quality of life in our contemporary organizations.

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About The Author

Harald Solaas obtained his degree of *Psicólogo Laboral* (industrial psychologist) at the Universidad Nacional de La Plata in 1967. He performed as a company officer in several companies and in various capacities for many years after graduation. His most significant experience was in SHELL C.A.P.S.A., local Argentine company of the SHELL GROUP.

He is in private practice as an organizational and HR consultant since 1991. He has conducted a variety of organizational programs in a wide range of industries and in governmental administration. He adopted RO theory as the theoretical and technical foundation for all his work in 1995. Since then he has been devoted to professional practice and theoretical research on RO.

Between 1997 and 2000 Harold participated in a large project in which Dr. Elliott Jaques acted as a consultant. In the course of this project and afterwards he kept an intense exchange of theoretical and technical material with Dr. Jaques. He has taught in several universities in Argentina, and currently hold the chair of *Análisis y diseño organizacional* at the *Maestría en psicología empresaria y organizacional* at the Universidad de Belgrano.

He has written a book on the evaluation of individual potential and a large number of articles published in several magazines and journals.

The Author may be reached at <has@impsat1.com.ar>





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Global Organization Design Society
32 Victor Avenue
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