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COMMENTARY

A Commentary on Kugler, Turvey, Schmidt, and Rosenblum's Ecological Approach to Motor Systems

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I argue that Kugler, Turvey, Schmidt, and Rosenblum's (1990) attempt to find adiabatic invariants is premature and describes several prior considerations that must be taken into account to properly understand a motor system. I maintain that Kugler et al. can and should be much more direct in taking the thermodynamic dissipative systems perspective on motor activity. For example, performance in a true "comfort mode" should be something that can be maintained "indefinitely" in practice for at least 3 to 5 hr. Then, it can be expected that thermodynamic equilibrium or near equilibrium has been established. Even if this is done, however, the fact that animals have alternative optimizations available to them and choose what they optimize in a given performance raises problems that Kugler et al. have not addressed.

In a second part of this article, I outline more specific issues concerning flow that Kugler et al. should confront and suggest how they might proceed. Finally, I explain why I am especially eager to see them investigate locomotion as against upper body activities and describe the sequences of experiments that should be performed.

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AN OVERVIEW AND HISTORY OF THE ISSUES

Asked to review the article by Kugler et al. (1990) in the previous issue, I suggested that it would best serve this community of interest to comment publicly on their general approach. In their article, the authors asked that: "The summed evidence . . . be judged conservatively . . . [it] is suggestive but far from conclusive." I add reasons for the caution they express.

I am a rather simple spoken physicist, so I have to voice my opinions accordingly. I will not begin with details because I want to cut to the heart of their search for insight. I cannot make much out of their adiabatic invariants. I think it is too "highfalutin" an idea that is being invoked prematurely, before there is an extensive body of experiment and theory to justify and carry such concepts. It is not because I object to them finally finding or arriving at some sort of conservation principle, but it takes a great deal of experimental and theoretical demonstration to show such a principle operating.

Varieties of Motor Optimization

Within the organism, two systems are coupled: the muscle fibers in the motor system and the vascular supply system. Without that perfusion, the motor system could not operate. However, it requires both the nervous and endocrine systems to couple the motor and vascular system. That third regulatory complex of the nervous and endocrine systems is also coupled to many other internal systems and even to the exterior world system. I am more than willing to accept or urge such a description as the model depiction for an ecological realism.

In the most naive physical model of the working of the system, say at some motor task, the nervous system, in its motor–sensory connection to the entire world inside and outside, "assigns" a task to a particular motor system. In the simplest sense, to assign such a task means to prescribe a velocity or rate of operation. The vascular system, then, as a follower system, acts to sustain the task. The most rudimentary biophysics of this process is that the subsystem velocity V is assigned. However, this assignment does not determine the power that has to be expended. That depends on the prevailing force F out there in the environment (e.g., the force required to walk up a hill or on rough terrain). Thus, the power P expended will be $P = FV$. The metabolic power will be furnished by the muscle engine as an oxidative engine that has to be supported chemically by the oxygen flow Q_{ox}, $Q_{ox} = P/h_{ox,}$ where h_{ox} is the heat of combustion of the existing fuel supply (e.g., free fatty acids, glucose, or over a long term, protein). The oxygen flow will be delivered by a Fick diffusion system involving the current blood supply Q_b to the muscle, $Q_b = Q_{ox}/\Delta c_{ox}$. Δc_{ox} is the current uptake of oxygen from the hemoglobin carrier in red cells (the arterial–venous concentration difference of oxygen).

In this schematic, to support the task, the vascular system has to provide sufficient blood flow to afford the force F, $F = h_{ox}\Delta c_{ox}Q_b/V$, thus making up the dual F,V that will maintain the task. For any general task, the P,V level is not assigned. Tasks may be conducted in any large but limited range of incrementally increasing P by the organism. The purposeful selection of that P, or V, is a matter of the organism's psychology; the matchings become a matter—if you will—of ecological psychology.

In military tasks and in sports, some kind of explicit "optimal" assignment becomes a matter of significance. Illustrative tasks, for example using arms and legs, may vary from traversing distances of 60 yards (or meters) to 26 miles or more. A recent extreme, a 168-mile run, was particularly noteworthy because it was achieved in 24 hr. Such sports tasks, except for the latter case, are all moderately well explored by humans (see any recent almanac for sports records). Obviously similar tasks and other kinds of task complexes exist for other mammals. Let us characterize a few diverse optimization problems associated with such tasks by biophysical reasoning.

Consider running the mile. The record time for speed has changed by perhaps a quarter in a century. That change is not merely a matter of metabolism. It is a matter of biophysical-physiological coordination. But that does not clarify the problem. I use my own experience for some clarification. I swim a mile a day. My style is "gorgeous Victorian." It impresses maiden aunts but exasperates any competent swimmer. I am not out to break any records, but all competent passerby swimmers indicate that they want me to swim faster. I really do not want to. I am willing to swim "better" for the same effort. That already means there is more than one optimization possible.

I go on to another task. The Army would like to know optimal load, optimal distribution of load (say for a 60-lb pack), and an optimal walking pace for such loads. I have a related but different problem. If I am dumb enough to want to carry four bags of produce and nine books from the library for three blocks from the store to my house, then (a) what is a tolerable distribution of those awkward bundles, and (b) how shall I pace my walk? Again a variety of different optimizations may exist.

Such test conditions raise a host of problems in locomotion and its achievement. They mix biophysical, physiological, and psychological problems. These problems are not clarified by tossing out a term, *adiabatic invariant*, for some very narrow case. There are a host of couplings that must be understood and explored broadly before one can narrow down on any principles of optimization.

Defining *Comfort*

I claim that I was the first one to provide a real physical measure for a complex psychological state—a definition of *comfort* in the perception of the thermal state of the human resting or exercising in the cold or warm. This was done as part of a project to develop a measure for the adequacy of clothing to maintain comfort (Iberall, 1960). This project was, in turn, part of a larger program of research and development work done on human safety equipment for high altitudes (e.g., respiratory equipment, space suits, and thermal insulation provided by clothing) and on the systems' physiology of the mammal (e.g., Iberall, Schindler, & Cardon, 1973, for a partial bibliography).

The analysis started out from a recognition that (a) the organism is a complex nonlinear composite of mechanisms and (b) the problem is thermodynamic. Just those two assertions, in themselves, were likely to assure the results that, because of the nonlinearity, there is more than one operating state, and that, because of the fixity of the machinery and the differentiation in tasks, each operating state involves a different thermodynamic engine configuration. Suffice it to say here, without much detail, that we established at least two operating poles of behavior of the mammalian organism or—more particularly—of the human. These two "pure" limiting states were a comfort mode of operation and a survival mode of operation.

In the survival mode, an animal remains quiescent, locked into a nonmoving state, regardless of the ambient environment. The metabolic response of the human (e.g., nude, shaded from direct sun, low wind, low humidity environment, reclining) for such a mode is a nearly flat metabolism from perhaps −5°C to +40° C. This metabolism can keep core organs (e.g., brain, heart, liver, and kidney) at nominal deep body temperature near 37° C, but it cannot maintain uniform surface temperature; at one limit, extremities chill to ambient temperature, and at the other limit for humans, heat evaporates from all surface areas.

We discovered that for a person to be comfortable, activity of the body and its attendant metabolism is required to maintain an average skin temperature of 33.4° C ± 0.15° C, with no region of the skin less than 30° C or more than 36° C. At the two extremes, physiological mechanisms cause a break in function: at the lower level, loss in near surface heat transport via blood vasomotion as a result of capillary constriction; at the upper level, the onset of sensible perspiration over particular regions. Body comfort can be maintained with these conditions prevailing, independent of the ambient temperature.

In order for these results to be meaningful operating states as they were clearly shown to be dynamical states, we had to establish the time scale for the existence of thermodynamic equilibrium. We showed this in the human as a 3-½ hr period, with related scaling appropriate to animal size. This means, among other things, that activity states like a 26-mile running marathon or a very surprising continuous 168-mile run are indicators of near equilibrium performance because such activities are clearly steady state tasks conducted long enough to test thermodynamic near equilibrium (a 100-yard dasher, a 1-mile runner, etc. would have to slow down to marathoner's pace to come closer to appreciating near equilibrium operation; or a marathoner would only have to moderate his or her pace a little to continue on to double or triple the run, e.g., from an average of

12 mph to 7 mph). These performances are not at peak heart rate or power or oxygen consumption, but are more like 60%–70% of peak.

Having established the body's equilibrium time scale did not, however, solve the problem of characterizing the comfort state. What we had to confront, in testing task operation over 3- to 5-hr equilibrium time scales, was why the person knew, after a short time of exposure to the task in an environment, whether the operating state would be comfortable, long before we could detect the person's state with very sensitive instrumentation. What did the body use as indicators of its comfort state? Once we had discovered the key indicators, that is an average skin temperature of 33.4° C ± .15° C and a range of 30° C to 36° C, then we "knew" right from the beginning whether he or she would be comfortable long before the person in the experiment.

An external operating motor state was suggested. Given that state, the internal cardiovascular state was determined as a follower. The cardiovascular system "informed" the organism as to its psychophysiological state. If the organism did not give command control to me, its "psychological" systems would likely then have elected or modified the operating motor state. But the organism did give to me command control (i.e., the person literally "danced to my tune"). (We used a large variety of music record tempi to drive the willing person into the generalized motion of a walk–dancing operating mode to change the metabolic level.) And we elected, from outside, to run the system at what we had learned it would identify as "comfort" (i.e., average skin temperature near 33.4° C).

Four Systems and Four Problems

How many systems are involved? At least four. There was the operating motor system and state (which could be psychologically controlled); there was the cardiovascular system and state (which could also—in part—be psychologically controlled); there was an "autonomic" central command-control system (which could "automatically" operate these two systems); there was at least some component of a "volitional" command-control system. I have no competence (at least at this time) to resolve the issues of command-control operation of that fourth system, so I will confine myself to remarks about the other three systems.

Given that the "volitional" central command system has initiated a motor task, what is involved in setting up that dynamic pattern? Here we deal with two different problems. The first problem deals with the setting of the volitional startup. The second problem deals with setting up the dynamic motor pattern and is the one that Kugler et al. (1990) attacked, but not adequately.

A third problem is how much actuation or support, or what sort of pattern, does the autonomic system supply to the cardiovascular system in support of the motor system pattern elected?

Given that the volitional system has elected to sustain a task (e.g., to reach thermodynamic equilibrium and beyond if necessary), we have to confront the dynamics by which the motor system achieves the task, the dynamics by which the autonomic system supports the task, and the dynamics by which the cardiovascular system also supports the task.

I have no objection to the way that I think Kugler et al. approached these issues, although I have no sense that it is unique. It is not unreasonable to assume that—if the system is working within its aerobic limit in which a thermodynamic equilibrium can be achieved—then the cardiovascular dynamics is modestly determinate, although I believe a considerable number of remarks are required to clarify that problem of near equilibrium cardiovascular dynamics (see, e.g., Iberall, 1974).

In Iberall (1974), I showed that every physiological variable had at least three steady state components: a genetically determined component; a "status" component, determined by the motor practice or other experiences of the past month or so during which architectonic changes in the cardiovascular system and other organ systems would take place; and an operational component, which depended on the level of activity under study. Each of these three steady state components had a dynamic spectrum independent of even other operational responses such as their transient states. Now this clearly suggests that the cardiovascular election has to be within the convenient operating domain of that system.

A fourth problem arises from observations that animals adopt "favorite" cardiovascular system responses, "habitual" performances that do not seem physically necessary. To the extent that such "habits" exist, we have the problem of how animals elect and maintain them (a start on this problem may be found in Iberall, 1989). I recognize that such habits exist both from the Type A–Type B personality of popular psychology, from a more general behavioral ethology, and from my model of how the adrenals are capable of biasing the hypertensive response in humans (Iberall, 1979). Also, it is clear in pets and in zoo animals that their activity elections are not the same as elections by the same animals in the wild. Why will an athlete exercise vigorously every day (or why will I swim a mile every day; or why, in the self-selection of those who stick with a vigorous exercise program, is it that only about 2% of those who enter the programs continue them for long periods?), I cannot tell you, but there does seem to exist a context in which animals elect and maintain, as a fourth volitional class of the steady state problem requiring solution, their favorite cardiovascular systems' response. An equally important result is that regardless of what elections are made, their average daily cardiovascular election is at low-duty cycle. So it is the detailed cardiovascular elections of tasks that are specific to a particular individual organism, not the general low-duty cycle election of that species or even that phylogenetic taxon.

To the extent that such elections exist and to the extent that each component of action pattern election is well practiced, I am willing to believe that one might then be able to identify something mechanical like an adiabatic invariant. I am then willing to believe that for a characteristic pattern of motor system elections in a given well practiced ecological milieu that the organism develops mechanical motor patterns that fit a reasonable thermodynamic efficiency that the characteristic cardiovascular system response will support. If this is true, then the animal does pursue some sort of mechanistic phase space response that will be "near optimal" for it in some sense (although I am not certain of the sense).

The Issue of Locomotion

Now we turn to the motor task of locomotion in animals. Nervous system elements that are presumably involved include the spinal cord, basal ganglia, thalamus, cerebellum, motor cortex, as well as various muscle groupings and their patterned responses. It is marvellous and truly elegant to observe them changing their motions with speed and task (e.g., from rest to walk, to trot, to canter, to gallop, and the like). But if one inquires whether these are simply mechanical switchings among instability domains, then I make the counterclaim that it is not yet determined whether those switchings are mechanical or thermodynamic nonlinear instabilities. I appreciate how much effort is going into demonstrating low-ordered nonlinear processes and the topology of those solutions, but I am far from convinced that high-ordered thermodynamic instabilities can be so represented. The typical cases in point, the Lorenz and other small dimensional sets notwithstanding (Abraham & Shaw, 1987), are: weather, the political economy, and the election of biological states. In all of these, the topology of Hamiltonian systems, including their possible chaos as a description of such natural complexity, still leaves me cold. The discovery of invariants among mechanical systems is much more advanced than the discovery of invariants among thermodynamically dissipative systems.

Thus, the election and switching of locomotion mode is, of course, a case in point. When Turvey and Shaw indicated to me that they were beginning to study motor systems some time in the 1970s, I had hoped and expected that they and their colleagues would get to locomotion quickly. I was pleased with Kugler et al.'s start on the hand's pendular elections. I hoped that he had elected a more generalizable locomotion task, but that is perhaps beside the point. The point now is to illuminate, dynamically and richly, the election of motor tasks, preferably common ones used by many animals, and the election of task modes that are common in their biomechanical life. This, I did not see in the Kugler et al. work.

Two Grand Coupling Questions

There exist a number of broad, central questions that have to be understood in physiological systems' study. Does the autonomic system in mammals make only well coupled deterministic elections in the operation of the cardiovascular system? For example, I have uncovered temporal scalings in that system at the 3-Hz level (as a tremor), at the 1-Hz level in humans (heart beat), at the 6-s level in humans (cardiopulmonary as well as nervous), at the 10-s level (oxygenation sensing), at the 100-s level (chemical vasomotion in capillaries), at the 7-min level (hypothalamic blood zonal control and reticular core), at the 20-min level (carbonate compartment relaxation time), at the 3-hr level for human size (cortisol drive), at the circadian scale (pineal, melatonin, and other nervous system components), at the 3-½ day scale (water compartment, which is likely regulated by the thyroid), and at the month or so scale (chemical-molecular turnover at the architectural scale)—just to name some of those scales that I know up to the status or architectonic scale. I know that the autonomic system affects and is coupled to almost all of these scales, but certainly not in the same uniform way. (Again, I will not hazard a guess as to how the genetic and "lifestyle" problem levels are coupled via the higher ordered volitional system.) So how are those connections, autonomic–vascular, made for motor tasks? Are they determinate, historical, developmental, or stochastic? I know they couple into, say, locomotion. But, as far as I can tell, they couple in all these ways.

The second question relates to the autonomic election of motor patterns. I know little about election of its status components except that they exist. I do not know its time scaled processes. I presume that those in rehabilitation and sports physiology and medicine may have some inklings about those scales (e.g., they may be scales between a few days and a few months).

But in any case, just these two grand coupling questions—how does the internal cardiovascular and motor systems (thermodynamic, fluid mechanical machinery and mechanical and thermodynamic machinery), each coupled to the autonomic system, couple through or at any of these scales?—still remains the basic problem for a physiology and physiological psychology, and, as such, at least one major component of a program for an ecological psychology of motor action.

Three Insights

To conclude this first set of remarks, I make my summary view by pointing up three distinct insights in my education. The first was the response to the Stark–Young (Stark & Young, 1964) article on the eye as a servo. My reaction was that I did not fully accept their mechanistic characterization, for if in the middle of the experiment, an attractive person of opposite sex passed into the field of view, I suspected that the observer would easily change attention from the pursuit task as a servo (just as the swinging arm is not a pendulum because it can stop its motion in midswing). Such tasks might have some limiting mechanistic performance, yet they may not be those mechanisms, really and demonstrably.

The second insight was when Jones and Milsum (1965) undertook the study of visual pursuit tasks on a variety of species. I called to their attention that the motor systems that could have been involved in their not very confined pursuit tasks seemed to have about 30 odd degrees of freedom, but the election of degrees of freedom involved in the actual pursuit trajectory seemed to be only about 6. I suggested that the narrowing mechanism of such choice seemed to be a very fundamental problem worth studying.

Now, in the broadest sense, inspired by the problems that Turvey and colleagues have chosen, but also by the question that McCulloch and I (Iberall & McCulloch, 1969) first raised about mode election in the 60s and by MacKay's even earlier attempt to call attention to, in the 50s, such election with his conditional probability matrix notion (see, e.g., MacKay, 1969) and also by the work that Goldberg and I inspired as a current ethological study (Gerstner, 1990), I hold in my mind the problem of what is the general action trajectory of a living sensory–motor organismic system in its action phase space and what determines those perhaps quasi-ergodic trajectories? These questions leave for a broader ecological psychology study.

BACKGROUND FOR A THERMODYNAMICS OF COMPLEX VOLITIONAL TRAINABLE SYSTEMS: WHAT DOES IT MEAN TO DEVELOP A THERMODYNAMIC DESCRIPTION OF AN "EXTENDED FIELD SYSTEM"?

I start with a few anecdotal problems that gradually brought me up to this central problem under discussion (see Iberall, 1989, for my current stage of progress). First, I start with a not-too-difficult flow field problem – the flow of, say, a compressible fluid through a nozzle or orifice into the atmosphere. The main take-away message I want this example to exhibit is the following (which is of governing relevance to the thermodynamics of complex systems and is already revealed in the simplest of flow systems): There is a field inhomogeneity – the flow field; it already exhibits at least two separated regions, one of which can be regarded as "interior" and one as "exterior," (an inside field and an outside field, respectively). Two different descriptions are needed for the two fields. For example, not uncommonly, the exterior field is "the rest of the world," with the attendant difficulties of describing that world. The process by which the two field descriptions (e.g., as laws of flow) are brought into concordance is itself a complex field process.

Turn now to the example of flow through a porous plug. If no external work is done by the fluid, the enthalpy of the fluid is said to be preserved. But that process is not reversible. In reversible thermodynamic theory, the problem is not resolved, and so although the initial and final stages of the expansion are said to be *equienthalpic*, the intermediate states cannot be identified. It takes an irreversible fluid thermodynamics of rather sophisticated form to resolve the issue of the intermediate states.

We catch a glimpse of that problem, still in the scope of reversible thermodynamics, in the case of flow expansion through a nozzle. Is that an equienthalpic process? It would appear so, but how is the actual flow process to be described? We separate out the expansion of flow through the nozzle from the more terminal problem. What is the nature of the flow in the expanding nozzle? First, it looks like a rather rapid mechanically accelerating flow process that might in fact be adiabatic (little or no heat can be picked up in the expansion). If reversible, such a process would in fact be *isentropic* (constant entropy). Such a reversible isentropic process is simple to describe and compute. It is described by such a law as: pv^{γ} = constant, $p^{\gamma - 1}/T^{\gamma}$ = constant, for an ideal gas, or slightly modified for a real gas. But because the flow is highly accelerated, what might one say about the reversibility?

Now it happens that a physicist colleague had been working on flow interferometry under Ladenberg at Princeton. It became clear, from his studies on flow nozzles (D. Bershader, personal communication, 1945), that except for the problem associated with a fairly thin viscous boundary layer at the walls of the nozzle, the flow (e.g., as perceived by its temperature field with distance) was quite precisely isentropic.

Clearly the irreversibility, even though in a highly accelerated fluid, was not in the nozzle expansion (except for the thin boundary layer). It took me a long time of study of the Navier–Stokes irreversible thermodynamic equations of fluid flow (see, e.g., Iberall, 1950) before I understood where the irreversibility occurred. It takes place, in this case of a nozzle operating sonically (i.e., in the expansion of a gas and its increase in velocity to a critical maximum) at the most constricted region of flow, the *vena contracta*. At that region, a discontinuous jump takes place in the fluid, in which entropy is conserved but the pressure and temperature jump. From that region on, as an expanding sonic shock wave, the jet then begins its imperfect spherical expansion with its extra component of forward momentum. From that region on the second half of the process by which the jet comes into its dissipative equilibrium with the outside world begins to occur. In that flow continuation, the ongoing process moves to restore the enthalpy of its entrance flow, but at ambient or near ambient pressure, once it has left the small vena contracta region. Its temperature still shows the porous plug equienthalpic jump in temperature, if the entire flow field maintains its adiabatic, no work, status.

So we see some beginning of complexity in this quite simple problem of attempting a real thermodynamic description in an extended field. We involved components that were "simultaneously" mixed—irreversibility, adiabaticity, isentropicity, equienthalpicity—all ideas that, if not properly unscrambled, would be contradictory. I will not bore you with more details that I had to uncover with regard to metastable phase changes, because I really had to do the problem with two phase expanding flow fields of saturated liquid and gaseous carbon dioxide and I first used the gas field as a beginning model before turning to two phase flows. Years later, I had to turn to making real porous plugs of very low porosity which modified the thermodynamics even more because the expansion was no longer adiabatic but more nearly isothermal (Iberall, 1953).

This, then, is the complexity present at the beginning when one begins to study fluidly mobile systems. Physics makes such problems tractible by unleashing a whole host of stereotypic simplifications, such as simplified boundary conditions, on them. But modern fluid mechanics and its thermodynamics has joined modern physics and no longer permits the simpler constraints. For example, nonlinearities of quite subtle nature arise in these fields at almost every point. And if the systems are themselves complex (e.g., presenting the "rest of the world" or any local region with a broad range of internal processes up to very long time delays), then the full nature of descriptive difficulty exists.

The Inside-Out Problem and Identifying the Comfort Mode

Kugler et al. (1990) did not address the inside-outside problem and the process of reconciliation between inside and outside. Let us start from the simplest of its aspects, which I faced in my thermodynamic studies of the human organism. My a priori estimate for the thermodynamic equilibrium of the human body, was about 3 hr (e.g., as judged by its settling time to thermal equilibrium). The first problem I faced was what kind of posture could I impose and maintain in a quiescent state to test for that equilibrium. It was clear that I could not maintain a standing posture for, say, 5 hr to cover a sufficiently long enough test period (for some reasonable modeling of the body's thermal losses, I needed a geometrically simple posture). I had to depend on the knowledge that for some portion of a time scale that has to be involved in an approach to physiological equilibrium, the body could apparently maintain a flat resting posture for a sleep period of 8 hr. Thus, I elected such a posture—an awake subject resting on a near massless net—for study.

If I were going to do calorimetry for the living human in that steady state posture, I had to decide what sort of calorimeter to use. The common laboratory pure modes of study are generally either an adiabatic calorimeter (thermal isolation from any heat flow and heat power measurement by temperature change in a standardized thermometric substance) or an isothermal calorimeter (constant temperature maintained in a wall enclosure with a heat supply and heat flow monitoring of flux through that wall). It is clear that active engine processes have to be tested in isothermal enclosures to avoid operational damage, such as inordinate raising of temperature.

I then had to find out whether a person could actually remain quiescent in the desired test posture in a calorimeter for 5 hr. What I had learned was that even small movements would significantly change the metabolic dissipation. (We were looking for energy closure at the percent or so level.)

Having established measures for such a base (little internal motor action at the margin at which most of such activity, e.g., working activity against gravity force, was reduced by body support), it was then possible to go on to test other steady state operational states, such as sustained activity for 5 hr, to continue to test for the near equilibrium period and its steady state thermodynamic heat flux. This period remained close to the value we had established by theory, at about 3-½ hr, as an active duty cycle. As I pointed out, this finding led to the comprehension of a comfort mode and a survival mode of body operation, all within our isothermal calorimeter studies.

Note that we had not yet determined what physiological equilibrium was, but by beginning to explore various segments of operation of the organism within some envelope of normal physiological performance, such as rest and activity and eating, we gradually began to understand (a number of years later) that the first physiological near equilibrium period for the individual organism was likely the diurnal period. That is, we could comprehend that most of the body's physiological processes would be discharged within the time scale of an Earth day and that practically all of the various cyclic processes we measured shorter than and within the 24-hr time scale were effectively independent of each other, a measurement that we made and reported on which never seemed to fall within common comprehension. More precisely, we demonstrated little or no binary cross correlation between most cyclic processes of different time scales. The blind fascination with a "central circadian clock" precluded attention by circadian investigators and others to that finding which I have been reporting from about 1952 on (see, e.g., Iberall, 1960, 1986).

To cite perhaps one of the many related and pertinent controversies that we had to deal with, consider our arguments with the Yale–Pierce Lab group of investigators of the process of thermoregulation under Hardy (American Institute of Physics, 1939) and his successors (e.g., Nadel; see Nadel & Bussolari, 1988). In our model (Iberall, 1960, 1972, 1986; Iberall & Schindler, 1973), we made clear that the mammalian thermoregulation was a dynamic regulation by thermodynamic engine processes, rather than a feedback control process around a hypothalamic setpoint in temperature which they believed in, and that it was necessary to appreciate the existence of a near 3-hr period for thermoregulation of its dynamic duty cycle rather than an on–off switching of metabolism, which had been provided by Hardy and Dubois (American Institute of Physics, 1939) as a classical picture of thermoregulation in some short run. Much of the classical picture can be found in that American Institute of Physics reference (1939, see chapters 5 and 6; therein, the work of Winslow, Gagge, Hardy, Hardy, and Dubois). Thus we tried to indicate that the Winslow–Gagge data base and the Hardy data base of metabolism and the avenues of heat losses, which was the accepted view of whole body operation, was not equilibrium data. We argued that the engineering heating and ventilating community had finally grasped that point (see, e.g., American Society of Heating and Ventilating Engineers, 1960) and that the physiological model impeded understanding of the real way that the mammalian system achieved its feasible long-term thermoregulation (e.g., how much air conditioning power was required for say steady state theater comfort, one of the practical problems that launched physiological thermoregulatory study). Our conflict was never resolved (see, e.g., Hardy, 1961).

Yet when the Hardy successors came on the scene as the physiological support for the long-term flight problem of the human powered Daedalus project, including how to best dispose and use and train humans for the task, their experiments and findings had to go over the same grounds that we had tried to establish in the literature for a background 25 years earlier (see Nadel & Bussolari, 1988). Their work is reported de novo (e.g., "Since there is practically no literature on the physiology of exercise that is prolonged for more than 3 hours . . ." p. 354), as if we had said nothing of any physiological relevance in the physiological literature any time before.

Perhaps, for the first time, they began to appreciate steady state operation at the aerobic limit. Reading their experience will confirm and enrich much of the message we are trying to send. Regardless of how they trained their athletic human power supplies, the actual optimal operation had to be conducted at the limit of steady state near thermodynamic equilibrium.

Reasons for Choosing Locomotion

We move on to the problem of dealing with a comfort mode of operation within the physiological equilibrium period of 24 hr, noting that if it exists, it has to be within the aerobic limit and it has to be maintainable for at least the thermodynamic near equilibrium scale of 3-½ hr. Clearly, that is one of the reasons that I felt that Kugler et al. ought to have used a locomotion task. Mammals locomote regularly. And the 168-mile run indicated very sharply what the limit was for such performance (i.e., perhaps 3-lpm oxygen uptake sufficient for the aerobic mode, with training, at 7 mph on a flat surface for 24 hr).

First, having investigated various modes of developing metabolic power in the human, I showed (Iberall, 1960) that appreciable external power could only be extracted from the lower torso. This is consistent with humans being forest and savanna animals accustomed to upright hind limb (i.e., leg and torso performance) rather than high level, front limb (e.g., brachiator, extended arm) performance.

Second, I had already showed that metabolic costs could be easily and accurately determined from outside (later we began the task of determining regional utilization and theoretical confirmations inside a small animal by more difficult "wet" measurements of oxygen and fuel uptake); for example, I could measure metabolic performance with what we called *G-string accuracy.* I showed that we could measure the whole body difference, say, of adding as little as a small square of cloth on the surface of the body as insulation, or we could measure the metabolic cost of a small change in body motion.

Note that in the Daedalus program (Nadel & Bussolari, 1988), the issue of comfort and persistence immediately had to be faced. They had to review, once again, what sort of posture the body could "comfortably" support and maintain a "high" value of external power or rate of work. Although it turned to bicycling (e.g., not weight lifting with the upper torso), they had to research an optimal position, which if you examine closely, is quite close to a walking-like posture. Loosely speaking, this involves determining what sort of individual motor pattern would produce the greatest work output say for the same thermodynamic steady state near equilibrium. This sort of election is like learning that a backward high jump can produce a desired "higher" jump than can a forward jump. That is, whether they understood it in such terms or not, they had to begin to search out a more efficient thermodynamic operation of the total machine at a near thermodynamic equilibrium limit of operation which turns out, again, to be near 3-lpm oxygen uptake; the general type (species or body type) and the specific individuals who might be suited for the motor task; and, finally, a training program for those individuals. These are all the kinds of issues that our studies had laid a foundation for a generation earlier.

Improving Kugler et al.'s (1990) Methodology

So now I turn to the problems addressed by Kugler et al. Whether they grasped the point conceptually or not, they really had to elect a "zero" body posture, one that is "comfortable" and lies within the physiological equilibrium range, which can be thermodynamically determined. I do not think that unsupported arm posture meets the requirement; I do not even think that standing erect is suitable, but that is part of a complex issue. We can avoid the more complex questions by electing near resting postures that can be sustained for long periods of time.

I briefly clarify the more complex problems by the following remarks: The resting (basal) metabolism is, say, perhaps 1,600 kcal/day for a normal man. The common average daily metabolism, activity and all, is a low-duty cycle value of perhaps 2,300 kcal/day. Thus one sees not too great a difference (25%) between nominal rest and a nominal average activity schedule. One may surmise that it is not far from wrong if one elects a fairly near resting position as a base, even if it is not "perfectly" basal. Some such postures could be a comfortable seated position (we designed such a seat, something like a modified motorcycle seat, for long time occupancy, for use in a physiological spectral analyzer—see Halpern, Young, & Ehrenberg, 1968; note also, as another example, that in our space suit research our test participants were often in our suits for 48 hr and had to be maintained in comfort), arms in a moderately supported position with a comfortable body position, a standing position using a slightly tilted board for support, or a resting position on a suitable support surface (we designed such a netted surface for low thermal interference in our early metabolic studies).

For the Kugler et al. studies, a position relevant to locomotion would have been most suitable, for example, the standing against a slightly tilted support surface, or a comfortable seated or part seated posture for leg or arm motion studies. For testing the delivery of large power, we found arm motions in the human to be quite problematical, just as it might be to determine optimum output for a first forefinger.

Now for a base, one would have to assess the total metabolic cost of maintaining that rest position and perhaps comparing it with a basal or minimum rest position. This, at least, would assign an internal metabolic cost to the muscle systems involved in maintaining the "rest" posture. In other words, Kugler et al. did not assess the metabolic cost of holding the arms in their base position as compared with a rest position, which would have determined whether their base position could be maintained to thermodynamic equilibrium, which would, therefore, also have determined whether that base position was "comfortable."

Had they determined that metabolism (the metabolic difference from rest, say), they would have had some sort of estimate of their E_o, which is the dissipative cost of maintaining the support engine (e.g., a muscle pair) with little or no external output, the starting point of their adiabatic invariant studies.

Given that base value (e.g., standing, sitting, and reclining) for the relevant motor system, one could then assign and begin to test a motor task (e.g., "walking"), some motional pattern appropriate to sitting (e.g., arm, torso, or leg exercises), or a motion pattern suited to reclining (e.g., bicycling). I obviously think that a locomotion task is most suited for mammals.

If one starts on an external motor task, it seems clear that any or all of the small amplitude tasks are performed as mass–spring–gravity systems (neither the gravity spring nor the muscle springs can be conveniently disconnected except in low-gravity environments or with extreme physiological interventions) and that most of them can be done with no external work, (e.g., by lifting and replacing weights) or effort against low friction systems (e.g., in our early studies, we first developed a frictionless servo load system for exercising arms and legs to change the metabolism that those motor systems worked with). In any case, it is plausible to see that near sinusoidal small amplitude motions can and will be achieved (i.e., pendular mass–spring-like systems).

Now in almost all of these cases, if the support is withdrawn for some part or whole of the body, then one can easily determine what mass or moment of inertia is involved in that "freed" system, and by, for example, a gentle tap, one can determine what a small amplitude "fundamental" motional period might be. It seems clear that these (mass, moment of inertia) are two elementary empirical properties of the subsystem involved. This result would seem to be very insightful because it represents two external measures, mechanical results, which the organism and its subsystems put forth into the external world. From these results, it is then simple enough to assign an elastic stiffness to the subsystem (e.g., its mass and spring stiffness or its moment of inertia and rotational spring stiffness) always a composite of mass, its distribution, elastic springiness of muscle pairs and an associated gravity spring (e.g., associated with raising and lowering of a center of mass). By conducting the analysis carefully, the muscle spring can be discriminated from the computable gravity spring, so that the internal engine can be identified and assessed.

Outside of a small nervous tremor (e.g., 3 Hz), another subject of study, the resultant pendulum-like motion, due to tapping of the freed system, is mostly considerably damped and with little reason to expect the muscle pairs to be linearly damped (in general, it may only swing once, quite commonly as a motor reflex, or perhaps a few times). So we really cannot treat the system as an autonomous external mechanical system. We are forced to consider the interior machinery, the entire thermodynamic machinery (see Iberall, 1989).

Requirements of Autonomy

If the system were autonomous, it would likely have two internal requirements, as long as that autonomous system could be made to continue to operate at that nominal response frequency. It would have to have an escapement of some sort (an impulsive or more continuous source of energy or force or power to make up for the damping loss of each cycle). This does not mean that it would operate in a limit cycle. So now we have to do piecemeal experiments to determine properties that characterize autonomous operation. These mixed pieces may not be so totally pure.

We might ask the subject to give us a "comfortable" small amplitude swing at the observed reaction frequency. At this point, it is clear that we can no longer avoid internal issues (if you want that issue to be spelled out most drastically, the subject can just refuse to swing or actually have no control of such a swing).

Clearly, all reactive swings effectively involved a nervous response of some sort. In some sense, antagonistic motor pairs are involved and there is an action–reaction around some equilibrium point. The metabolic support supplied by the cardiovascular system is not far from a rest value; unless the amplitudes of disturbance are very small, a nervous response at above the capillary level is evoked. Thus the existing stiffness of the motor pair is elicited. A priori, one might guess that responses in the "small-fraction-of-10 Hz" range would be expected because a nominal 10-Hz response of all nervous systems. The actual response would depend on how many component series-parallel "functional unit" motor systems are involved in the pendular subsystem (see, Bloch & Iberall, 1982). At this point, we are not prepared to go into the emergent evolutionary design of appendages in mammals that consist of design appropriate to innervated muscle pairs, appendage masses, and their relation to the gravity load and springiness. Suffice it to say, that a combination of these factors are involved. These appendages are proportioned so that their muscle pairs can deal with the gravity loads through a comparatively universal nervous system.

However, when the body is asked say, to maintain that frequency by being given that frequency as an orchestrated rate from outside, it no longer exacts the same innervation. Some motions cannot be comfortably maintained. The problem has to be assigned to the subject and tested. Suppose we find such a motion (it is little wonder that lower torso muscles are so commonly found apt in humans). If the subsystem finds the motion convenient, then we could test its metabolic cost at a variety of amplitudes at the prescribed frequency. Suppose that the system were nearly ideal, which would mean that it was an isochronous mass–spring (an oscillatory frequency independent of amplitude, requiring a linear spring) and an impulsive escapement, under which case the motion would be a limit cycle motion, then we could surmise what the response would be. It would be that of a constant Q system. In that case, what one would find is an internal metabolism which grew linearly with the amplitude. The incremental energetic cost (above the rest state) would not be a measure of the escapement energy because the cost of supporting the escapement in the vascular system would also be involved. But if even a crude measure could be made of the damping from the impulsed decay, for which an a priori estimate would likely be critical damping, then we would have some idea of what it cost the vascular system to run the excitation for the nervous system escapement. Remember that there is no a priori reason to expect that the system acts like a limit cycle system.

If we have the metabolic cost for running at a near-fundamental frequency with changing amplitude, then the effect of driving the system at other prescribed frequencies can be investigated. For example, the Kugler et al. experiments can be done swinging the leg (either sitting or standing). After a fundamental is established and its metabolism determined, then the leg can be loaded (e.g., by heel plates or by distributed weights), and these new fundamental periods can be investigated. Then the Kugler et al. experiments can be done (e.g., driving one foot at the unloaded fundamental and the other loaded). At this point, the interesting issue of locomotion can begin, for example, one foot walking on one driven treadmill and the second loaded foot on a second treadmill. With experiments like these, the mechanical and thermodynamic invariants can then be properly studied.

All of these responses so far are simple autonomic responses, because the motions so far still appear to be simple. It is at this point that the more complex "learning" tasks (learning what the motor systems can do as complexes of motion) can begin. For example, if now the one foot or two foot problem is approached on single or double treadmills in which the problem is to lower the metabolic cost—for the same frequency—by beginning to put some "body English" on the body (e.g., besides swinging the legs, tilting or rocking the body, and using the arms). Here I do not know the results, but the problem is, given the major motion to be a leg swinging pattern for say "walking," are there minor complex body motions that will reduce the metabolic costs? This does not solve the athlete's problem of rather arbitrary body motions in a complex configurational space (e.g., jumping a hurdle and twirling a hula hoop), but it begins to get at it. Then we can address the problem of higher ordered optimizations of tasks in life (e.g., making love, making war, or simply making do).

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