Communicating interactivism: An introduction to the Library of Ecological and Interactivist Studies

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Introduction

The failure of interactivism and ecological psychology to establish a dialogue, for their proponents to collaborate or influence one another in any lasting or meaningful way, is one of the great intellectual tragedies of the last 50 years. I can only begin to imagine what might have come from a collaboration between Mark Bickhard and Bob Shaw, ecological psychology's great sage, whose work, I feel, is woefully underappreciated (see Shaw, 2001; Shaw & Kinsella-Shaw, 1988, 2012; Shaw et al., 2019, for instance, for his work on intentional dynamics). Here we have two people working on very different problems, but still within a broadly overlapping framework, and with a deep enough appreciation of fundamental physics and its mathematics that maybe, just maybe, they could have moved us closer, synergistically, to a mathematics with enough dynamics for the problems of life and mind. Or not, maaybe I am just too idealistic. Ecological psychology is not alone here. Among some of the other radical psychologies on offer at the moment, interactivism has a bit more influence, though it is still quite limited. For instance, we have seen considerably more engagement by the enactivists some of whom have borrowed interactivism's technical language, like 'recursive self-maintenance' and acknowledged that key components of the interactivist model deserve consideration, namely, that far-from-equilibrium thermodynamics fit in *somewhere* (Bickhard, 2016). Terrence Deacon's work on 'autogenesis' is perhaps as much informed by interactivism as a concept can be without *just being* a project of interactivism (Deacon et al., 2014). But most of the rest of radical psychology has given little more than lip service, if engaging at all, often no more than a confused reference or footnote (e.g., Chemero, 2011 Chapter 3, Footnote 8). And the relationship between interactivism and mainstream psychology is nearly non-existent. Ecological and enactive psychology have had a bit more influence in that regard. The mainstream maintains a hazy recognition that embodiment is a thing that might matter somehow. And concepts from dynamical systems are increasingly popular, thanks to their introduction by pioneering researchers in the orbit of ecological psychology back in the 80s and 90s: Bruce Kay, Scott Kelso, Peter Kugler, Linda Smith, Dagmar Sternad, and Ester Thelen, to name a few (Haken et al., 1985; Kay, 1988; Kay et al., 1987; Kelso et al., 1981; Kelso et al., 1980; Kugler et al., 1980; Kugler & Turvey, 2015; L. Smith, 1995; L. B. Smith, 1995; L. B. Smith & Thelen, 2003; Sternad et al., 1995; Sternad et al., 2019; Sternad et al., 1992; Sternad & Turvey, 1995; Thelen, 1995; Thelen et al., 1993; Thelen & Smith, 1996; Thelen et al., 1991).

So *what* is the problem? What makes it so difficult to break through, either to our cousins among the radical psychologies or to the mainstream? I suspect we all know the answer regarding radicals. You probably do not end up being a radical if you are not a bit like a cat in your independence, and herding cats is notoriously difficult. But I also suspect that Kuhn's 2021 conception of 'scientific revolutions' looms a bit too large, whether in the somewhat nihilistic attitude that 'you have your paradigm and I have mine, nary the twain shall meet', or in the hope or fear that some radical psychology or other will take over and become the mainstream. I do not believe in scientific revolutions. I think bad assumptions big and small are discovered every so often and over turned, but even then, change can be painfully slow and incomplete. To borrow some text from Timothy Cauller's 2012 doctoral dissertation (committeed by Mark Bickhard) regarding educational reform:

> While these reforms paralleled the rise of a much greater diversity of constituents in the national educational discourse and greatly affected, as well as continue to affect, educational priorities in American schooling, it should be noted that a number of elements of previous templates of educational progress remained rooted firmly in place. [...] in spite of repeated, vociferous attempts of reform advocates over the decades, no one approach to schooling has ever completely supplanted or eliminated any other approach. [...] if anything, competing approaches are more often temporarily submerged than replaced. pp. 26-27.

Cauller's discussion of school reform makes for an apt analogy. Later, I will discuss the incredible edifice that is interactivist theory. I believe this edifice is its greatest asset,

but such an edifice can be quite intimidating to behold it with naïve eyes, especially for the kinds of unruly 'cats' with a strong sense of independence. Cauller goes on: 'Educational reforms imposed from above or from without that ignore the organizing grammatical system of schooling, and which promise to change completely the way that schools, teachers, and the system operate, are doomed to failure.' This is because existing 'educational philosophies and practices likewise have evolved over time and have underlying structural regularities that support their effectiveness', in analogy to language, where the 'mostly invisible but organizing grammatical or syntactic structural regularities [...] have evolved over time to aid comprehensibility' p.28. Similarly we might draw analogy between learning a new theory and learning a new language, which is more than just memorizing vocabulary and learning grammar, it is learning a new culture. Thus, Cauller suggests that

> the reforms that seem to make the most difference and last the longest are typically the ones that build outward from the inside of the system and fully recognize the controlling power of the grammar of schooling. The potential for the success of a given set of reforms depends to a large extent on taking into account the opinions, practices, and aspirations of all constituents who have daily responsibility for seeing that Americas youth are being provided with an education that meets their particular needs. Only then can societys new goals for an educated citizenry be achieved.

Think of Tolman and his early attempt at operationally defining mental processes in terms of observables Tolman (n.d.). I might define my cat Gandolf's hunger in terms increasingly difficult to ignore nuisance activities, like knocking things off my desk and chewing on cables, all this followed by increasingly loud meows and darting around in front of my feet as I walk toward the kitchen. Or I could have to define my cat Ríoga's hunger as hiding from me and fleeing when I try to pet or snuggle him, scratching if I succeed in catching him, and again, increasingly loud meowing as I move closer to the kitchen. As silly as this might sound, Tolman's later conception of a cognitive map guiding mice when running in mazes is what made the move back to mental phenomena more palatable to the then behaviourist mainstream. And behaviourism never really went away, as will be attested in my discussion later about psychology's philosophy of science.

The point here is that we cannot count on the old guard dying off, giving up, or being overturned in favour of some new radical thinking. Even if the physics of the early 20th century saw anything like a revolution (and really, they still teach classical mechanics for the first physics classes in college), today there are simply too many psychologists, and as I will argue, they need something to do, and the doing has got to pay the bills. Instead, in line with Cauller's arguments about educational reform, I believe we need to find a way to make the move to interactivism a move from the inside out, and to do this, we need to make interactivism more accessible to the newcomer. It needs to be easier and more natural to immerse oneself in the grammar of interactivism. This is why I have created the Library of Ecological and Interactivist Studies, but I will return to that later. For now, I want to explore some additional problems, the grammar of mainstream and to some extent radical psychologies.

The Problems

Psychology's Philosophy of Science

For much of its history, interactivism has been moving against the tide of experimental psychology and its philosophy of science. It was not so much that interactivism contradicted whatever it is experimental psychologists had to say, it was more that experimental psychologists were sceptical about saying anything at all beyond the narrow scope of their experiments. Radical psychology, in general, is always on the outs with experimental psychology and its antipathy to theoretical research. Even mainstream cognitive science, I suspect, has found its efforts frustrated in this regard. Consider Fodor and his arguments for nativism, which in the hands of experimental psychology became a cottage industry of infant habituation studies. Instead of a theory, we got a catalogue of ages at which different kinds of puppet shows elicited changes in 'looking' behaviour after a habituation regime. This example illustrates a broader attitude among experimental psychologists about the relationship between measurement and theory, a kind of inductivism which consists in gathering large amounts of measurements and looking for patterns. Technical terms get 'operationally defined' as patterns of measurement data, and theories as patterns of patterns of measurement data. This conception of science was hatched in the early days of positivism, based in the writings of Ernst Mach and dubbed 'Machian neopositivism' (Bickhard, 2001; Suppe, 1977). Suppe (1977) describes Mach's attitude as such: '[S]cience is no more than a conceptual reflection upon facts whose elements are contents of consciousness given to us by sensation' (p.9). If science is 'no more than' conceptual reflection on facts, then everything that goes into setting up those concepts to do the reflecting is something other than science. This attitude is not *unique* to experimental psychology—any 8th grader harbouring a nascent scientism fuelled by Neil deGrasse Tyson's podcasts would probably attest to the same belief—but experimental psychology has come to *exemplify* the attitude. The positivists themselves dismissed the idea not long after it was proposed, but apparently, experimental psychology found the notes and ran wild with them (ex: dust-

bowl empiricism).

The foregoing attitude is a problem for numerous reasons, which Mark Bickhard has catalogued extensively (Bickhard, 1992, 2001, 2017; Bickhard & Campbell, 2005). Even though inductivism is *supposed* by its proponents to eventually reveal a theory to the collective scientific consciousness, in practice, the measurement-centric attitude promotes nihilism, if not outright hostility, about theory. Indeed, the attitude seems to be 'the less theory the better'. Theory is regarded as a necessary evil, if that, at best 'useful' for obtaining measurement data. But the same can be said for a metaphor, and psychology has many metaphors. Indeed, even theoretical psychologists have a habit of regarding a theory's technical terms as no more than metaphors. In the past, metaphors have been very transparent about what they are, but today, it has become easier to hide metaphors in the language of computers and networks, with an unearned veneer of ontology that vanishes upon inspection. Another strategy is to make analogy to chemistry's 'table of the elements' with a list of, for instance, fundamental needs or personality traits. But unlike the principled, processed manner in which chemistry gets its 'elements', these are determined by a combination of common sense and statistical factoring. There is no process of differentiation to answer questions like 'why 5 traits, and not 2, or 6, or 20, or 100?'. So theory, metaphor, and lists of terms all end up with the same epistemic status: instrumental means for ginning up measurements.

But a theory has more content than can be accessed by measurement alone, and therefore more data than can be provided by measurements alone. For instance, 'Theory A accounts for more phenomena than theory B'. Or 'Theory A is more general than Theory B'. Or 'Theory A integrates the phenomena explained by Theory B and Theory C'. Or 'Theory A avoids Criticism K but Theory B does not'. Or 'Theory A doesn't render the emergence of phenomenon P impossible, but Theory B does'. And so on. But the measurement-centric conception of science renders all that data meaningless, and its proponents in experimental psychology have historically dismissed such data as having no value to science whatsoever. Consequently, theory itself gets side-lined, because 'it has no *more* to offer than anything else, and indeed, all the extra content can be misleading, because it deals in norms and metaphysics and cannot be measured. Just the facts, mam!'

Experimental psychology's nihilism about theory has several ramifications for the science itself, which I will discuss later, but it should be apparent at this point what its role has been in limiting the influence of interactivism and its fellow radical psychologies. A measurement-centric worldview has no place for theoretical research, much less ontological or metaphysical research. It has no way to make sense of the web of criticisms, constraints, and ontologies on offer by interactivist theory. This is akin to the flatlanders looking at the two-dimensional intersection of a cube passing through the flatland plane. But not all is doom and gloom; the situation is improving. First, not all of experimental psychology supports the measurement-centric worldview. Ecological psychology has one foot in experiment and the other in theory. University of Connecticut's ecological psychology program began as the 'experimental' division of their psychology department. And Gibson himself was conservative enough that he expressed uncertainty about whether his theory was applicable to aquatic animals, because his research was limited to land animals (Mace, personal communication). Yet despite Gibson's conservatism, some of his followers have devoted many words in publication to theory. Though admittedly, I have encountered instrumentalist attitudes among some ecological psychologists. Second, theory has been given much more room to breath in the last decade or so. Mark once quipped that there was a time when you could not say '2 + 2 = 4' without someone asking 'Where's your data?' Interesting to note, even social media culture is more insightful than that, with a recent controversy on Twitter about cultural approaches to mathematics and the question of whether 2 + 2 = 4 is as unproblematically and universally true as is often assumed. Instead of 'where's your data?', people are asking 'What are your assumptions?', a recognition that even the eponymous 2 + 2 = 4 comes with assumptions about just what kind of thing a 2 or a + or an = is and the context of its use (as when '2' is a floating point number in the context of computations in C++ or R or some other programming language). But these days, dynamical and computational modelling is much more common, especially in neuroscience. And the modellers themselves have been much more vocal about the need for more theory (Devezer et al., 2020; Guest & Martin, 2020; Guest & Rougier, 2016; Navarro, 2020; Pitt et al., 2006; van Rooij, 2019; van Rooij & Baggio, 2020a, 2020b). As useful as a model can be, models themselves are not really theoretical. A model's theoretical content is to be found in its use. A model is implied by theory, or theoretical assumptions. And it is always possible to infer contender models from apparent patterns in data, so model-centric science can be just as theory free as measurement-centric science. So perhaps psychology is in for a sea-change. Or possibly, psychology is in for another dustbowl, but with mathematical models in addition to the measuring sticks. Time can only tell.

Doables that Bring Home the Bacon

Why has the measurement-centric worldview been so popular? Part of it has to do with a cultural narrative about what makes an idea scientific and another pseudo-scientific or religious or philosophical, and so on. Science backs up its claims with data, concrete, objective measurements of the real world...like ratings on a Likert scale regarding an imaginary person's interpersonal warmth after reading a descrip-

tion of them with almost no content. Or maybe estimates of distance made while wearing a light or heavy backpack for a walk you have not been on. The real science, so it goes, is the measurements. And if that is the case, then what is theory for if it is not generating measurements?

This last question gets right to the issue. For much of psychology, measurements are easy to get, as long as all you look for are classical cause and effect relationships. Bonus for complicated interactions and mediators. In recent times, researchers have drawn upon methods from statistical physics, dynamical systems, and computational modelling, none of which requires all that much theoretical content, but they lend themselves to more theoretically oriented research, ecological psychology being a key example. One of the benefits of modelling is that you can relatively easily discover qualitative changes in a model's behaviour by finding its solutions or exploring its parameters. Till Frank, for instance, has had incredible success modelling any number of perception-action, psychopathological, and other phenomena with different solutions of the Fokker Plank equations. But beyond modelling hysteresis and bifurcation, making the move from theoretical to empirical hypotheses can be incredibly difficult. The situation is opposite for fundamental physics. Theory is cheap, and with something like string theory with many people working on it, so much structure already exists that finding a small, workable problem is easy. Turning any of that theory into experiment is an entirely different problem, not to mention the literal cost of getting the needed measurements. Smashing particles is not cheep. This latter example illustrates that narrative or no narrative, most scientists pursue whatever activity is most readily available to them, they want the doables that bring home the bacon. Interactivism's situation is similar to that of fundamental physics. So much structure already exists that theoretical research almost just falls out of it. Empirical research, on the other hand, not so much.

What does empirical research look like in the world of interactivism? One example is Jed Allen's research on the development of second level knowing and reflection (Allen & Bickhard, 2018). His research is a straightforward test of a major pillar of the interactivist model, which has something to say about how the child's knowledge changes as the child's brain develops. But interactivism might be able to shed new light on old phenomena, and the research need not be specifically interactivist. Take the priming paradigms used by psychologists to study the topological structure of knowledge, usually conceived of in terms of the accessibility of knowledge given already accessed knowledge. Priming paradigms are used by cognitive and social psychologists alike. Priming has been used to study the ordering of knowledge by superstructures like stereotypes and metaphors, like interpersonal warmth or distance. Measurement data generated in these studies tend to be highly variable, and the effects (if they exist at all) can be notoriously finnicky. The same priming procedure might work wonderfully in one lab, but move to another lab at a different school and you get nothing. In general, the effects are small and fail to attain for all participants in a sample—sometimes even going the wrong direction. The so-called 'replicability crisis' got its start with the failure to replicate some of these studies. Priming paradigms for more simple structure, like the relationships between concepts like 'doctor', 'nurse', and 'cookie monster'. If you saw a doctor and then either a nurse or the Cookie Monster, you would probably recognize the nurse as a nurse faster than you would the Cookie Monster as the Cookie Monster. Perhaps the problem is not so much complexity as it is the underlying topology, which governs relations of closeness.

Priming is often conceptualized in terms of the 'spreading activation' concept, which assumes a topology with very few parameters. The idea is that you have a network of representations, and whenever one of them is activated, the activation spreads to nearby nodes, activating those, and so on until the activation dissipates. Presumably, 'nearer' representations are separated by links that are either shorter or have greater bandwidth. The parameters are strength of activation (stronger activation spreads further, in effect shortening the distance between nodes) and whatever it is that changes the activation-independent distances, like number of times activation travelled through a link (Hebbian learning). As should be evident, the spreading activation model is very much limited, and not particularly dynamic. So why that model? Why not a more dynamic topology? Perhaps a chaotic manifold, like the Lorenz system where two points might start out very close, and then circle around the same wing, and circle around different wings, and then around the same wing again, maybe they become neighbours temporarily and separate in a quasi oscillatory fashion. Measurements would be quite unpredictable indeed.

Another possibility is described by Bickhard and Campbell (1996). Interactivism has a concept of microgenesis, a process of set-up, an ongoing preparation for the flow of interactions to come. An example of this might be coupled oscillators with coupling relations modulated by a set of parameters that are themselves dynamic. The values assumed by those parameters, whatever the cause, constitute microgenesis, setting up the course of oscillation to come. A given interaction may overlap to some degree with another in its microgenesis, as when the same parameters modulate several different sets of coupled oscillators. In this case, the interaction's microgenesis fully sets up the interaction and partially sets up others. The more overlap, the less partial the set up. The less overlap, the more partial, if not none at all. Overlap, then, defines the relations of closeness. Those interactions with more overlap are closer than those with less. If microgenesis crosses two or more timescales, as with parameters that change at different rates, then the lower

frequencies act as a context in which the higher frequencies change. The high frequency processes, then, might assume entirely different sets of parameters for different phases of the lower frequency changes. In effect, this would mean that interactions that are close during some phases end up being distant in others. The more scales involved, the greater the variability. Here we can bring back complexity: the relationships between what we know about, say, thermal and interpersonal warmth may be of higher dimensionality than the relationships between what we know about hospitals and Sesame Street. Even without candidate mathematical models, it would not be impossible to test some basic hypotheses about these topologies. But *with* candidate models, it would be possible to discover phenomena hidden in the math and look for those in the real phenomena.

Ecological Pscyhology

The foregoing discussion takes us now to ecological psychology. First I want acknowledge that while ecological is its own school of thought with its own traditions and its own 'grammar', if you will, Gibson's theory of perception is an interactive theory, even if not an *interactivist* theory. Without the metatheoretical stance against mental processing, even outside of perception, there is no contradiction whatsoever. Anti-representationalism, especially anti-encodingism, is perfectly appropriate for the study of perception, and I would argue that ecological psychology is no more and no less than a theory of perception, or technically, perception-action, it is unwarranted as a general proposition. At best, 'no representations' is a constraint that motivates creative problem solving. But I do not believe we have any in-principle reason to reject representation, or better yet, representing, in general.

Consider the perceiving-acting organism embedded in an ecosystem, which includes primary layout, and secondary energy fields given spatiotemporal structure by the primary layout. The weakest statement describing this structure as 'specific to' the layout is that the structure and the object are correlated. The organism, in interacting with the energy fields, does so in such a way that its activities become correlated with the light. In effect, the organism has picked up the *being correlated with* of the light and the layout, and thus *being* correlated with the layout. Contra Fodor's nonsequitur about a move from 'picking up that the light is so and so' to 'perceiving that the layout is so and so' there is no encoding of the light. The light is not perceived at all, except with regard to the layout being well lit, poorly lit, this colour or that, and so on. Perceiving, as stated by Michaels and Palatinus in their 2014 'A Ten commandments for ecological psychology', is *detecting information*. And as suggested by Michaels and Carello (1981), the information is not always specifying, which makes it possible for the perceiving-acting to not achieve its end. Specificity is what make regular, reliable perception possible. Without it, you would not find videos of people popping wheelies on the handrail of a cliff. Perception would simply not be reliable enough for that.

Why did I raise this issue after talking about priming and alternative topologies? Ecological psychology, under the influence of Anatol Bernstein and Michael Turvey's interpretation of him, has adopted the language of degrees of freedom. Examples of this can be seen in the concept of synergies, where degrees of freedom are mutually constrained to realize a singular coordinative effort, like walking, and transitions from walking to running or running to walking can be described in term of a change of synergy. The mathematics of dynamical systems is used to model the dynamics of phase transitions, which produce phenomena like decreased damping of fluctuation and increased time to settling in an attractor. The Bénard preparation a highly popular example, as it illustrates emergent order arising from an instability that collapses many degrees of freedom into very few. I think this example, while useful for some things, is deceptive. Organisms are not driven by head disequilibria, not usually anyway. And it encourages thinking in terms of mass action chemistry and thermodynamics. The intuition seems to be one of a bag of unrelated degrees of freedom spontaneously organising into a pattern.

Now, an organism's degrees of freedom are *already* organized; some of them are more prone to forming synergies with one another than others. This fact implies an underlying topology akin to that discussed above: When two synergies share most of their degrees of freedom, they are 'close to' each other, those with fewer degrees of freedom are 'further apart'. In a recent paper (Fultot et al., 2019) I proposed a model of 'multiple criticality' based on a paper by Wallot and Van Orden (2012). I suggested that many critical points exist, where a whole region of this topology is near criticality, so that many very similar synergies are available, and something like neural resonance associated with information pickup selects which of them undergoes criticality. Additionally there is a process of clarification about an environment's layout. The creature starts by differentiating aspects of the layout of very low grain, and learns to differentiate finer and finer grains. From the multicritical synergy perspective, this suggests a distancing, with fewer degrees of freedom in common between interactions with environmental layouts. So again, if we can identify an appropriate topological structure, I think we can do better than mass action chemistry. In part, because again, we can use its mathematical structure to discover phenomena hidden in the math, to be revealed through exploration and verified by observation.

Finally, let us address the issue of representation. I have introduced several related concepts: the picking up of the fact of being correlated, and synergies. With something like neural resonance, where for instance, the temporal structure of our picking up activities resonates with ensembles of fre-

quency modulated firing. This shows how processes further down the pipeline in the perception-action system can make use of the products of perception, the fact of being correlated. These products then serve as indications about what kinds of activities are available to pursue. 'Indication' here just means that the products open a path to visiting some portion of the perception-action system's topology. Transformations of this topology constitute a kind of cognition, or thinking, and its dynamics are constrained by the creature's ongoing perceiving-acting. It does not stand *between* the perceiver and the environment, as a classical representation would. Nor does it *enrich* perception in any way. If anything, cognition is enriched by perception.

Why it matters

I cannot say for sure that now is finally theory's time to shine in psychology, but I am optimistic, though it is far from certain to be the case. The big new thing in psychology is neuroscience; not the neuroscience of fMRI machines and ostensibly ironic phrenology heads decorating labs and offices. This is the field called 'neurophysiology' back when Pavlov and Bernstein walked the halls of Soviet academia. Its a field that could easily go the way of molecular biology, more engineering than science proper. Nervous systems are at least as rich a source of discovery as genetics, with new, surprising results coming out every day. Neuroscience could probably content itself to rummaging through nervous systems without a theory or a plan of any kind for the next century. Or they can follow the lead of people like Buszaki, Cisek, Friston, and others: the modelers. Cisek (2019) is notable for his call to throw out all language of psychology and replace it with a developed by and for neuroscientists, a language that begins with simple negative feedback processes and goes from there. Or Buzsáki (2006, 2010, 2020) and his models of neural resonance. But why stop there? Is Cisek's new language just another list of empirical phenomena, or an organization of processes with emergent properties and differentiations *into* the elements of his language? In other words, 'What's your ontology?' The answer to that, I suggest, should be interactivism.

Put aside for the moment the question of whether interactivism is ultimately correct. If you want 'useful', consider that with interactivism you get the following:

- A conception of naturalism that does not reduce to reductivism,
- A principled set of constraints on theory construction,
- A metaphysics within which to do theory construction,
- A non-rationalist, non-empiricist, evolutionary epistemology,
- A philosophy of science,
- A method of criticism and a way to make sense of criticism (not possible in the measurement-centric view),
- And theories (of varyingly schematic detail) of:
  - Normativity,
  - Functions,
  - Representing (multiple interrelated theories of representation),
  - Learning, development, and evolution,
  - Perceiving-acting (Gibson's theory with small modifications),
  - Pain, moods, and emotions,
  - Motivation,
  - Language,
  - Primary and reflective consciousness,
  - Goals, values, and morality,
  - Aesthetics,
  - Psychopathology and therapy,
  - Sociality,
  - Personhood, and others.

The edifice I have outlined above is why I became an interactivist. Indeed, it is why I became a psychologist, even though at that time I had never heard of interactivism. No matter the topic, there is probably a paper about it with at least a minimal sketch of how to move forward theoretically. Interactivism, as I suggested earlier, is a scaffold and a cluster of affordances: . a nesting of opportunities for more action. This is why I believe that of all the radical psychology on the table right now, interactivism is the only one that even comes close to being not just a 'real' theory, but whole paradigm.

Unfortunately, the very reason to be an interactivist, the edifice, is also the obstacle to being an interactivist. It is just so much. Where does a reader begin? How does the reader relate? How does the reader become competent to the grammar, so to speak, of interactivism? Diving uninitiated into a paper like 2009's 'The interactivist model' is incredibly intimidating. The first half of the paper is all criticism, and it is very similar to many other papers until it gets to the phenomenon of interest. I created the Library of Ecological and Interactivist Studies to tackle this problem. First, I am making Mark's papers available. This is the first hurdle, other than the papers available on his website. The next phase is to make tutorials about elements of the model, YouTube videos walking the viewer through something like implicit presupposition, internal relations, the three part model of content, and so on. The viewer can then peruse the website and pick up bits at a time.

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