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ORIGINAL PAPER

# Pragmatic Reductionism: On the Relation between Contingency and Metacontingency

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## Abstract

One of the main arguments in favor of the metacontingency as a model for explaining social phenomena is that it embraces another "kind" of selection (cultural selection) beyond natural and operant selection. Despite being "emergent" on operant processes, it would not be "reducible" to operant selection. Consequently, cultural selection would demand a conceptual framework of its own, hence the metacontingency. Assuming the existence of another "kind" of selection is an ontological premise, and that this new process requires its own conceptual framework, because contingency analysis is insufficient to explain it, can be considered an epistemological premise. Our goal in this paper is to argue that the epistemological premise present in the metacontingency literature is wrong. To do so, we present pragmatic reductionism as a model to discuss the possibility of reductive explanations of selection and maintenance of cultural practices from metacontingency to contingency analysis. Based on this framework, we provide examples of pragmatic reductive interpretations, thought experiments, and an analysis of experimental data in which we try to explain away the metacontingency. We conclude that it is possible to pragmatically reduce metacontingency explanations to contingency explanations. That does not, however, invalidate the ontological premise about the existence of different processes related to cultural evolution and selection whatever those might be. It only shows that, if they exist at all, they are not the ones being studied in metacontingency research.

**Keywords** Contingency · Culture · Emergence · Explanation · Metacontingency · Reductionism

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The assumption that the selection of cultural practices involves a new kind of process other than natural and operant selection is one of the main arguments commonly used to justify the metacontingency explanatory model. Needless to say, it has been pointed out that the parallels between natural selection and operant reinforcement raise their own doubts and arguments have been made of its usefulness (Tonneau, 2016; Tonneau & Sokolowski, 2000, 2001). Nonetheless, discussing cultural processes in terms of the selective metaphor led to three consequences in the culturo-behavior analysis literature. First, the "cultural world," or as Harris (1964) would have put it, "cultural things," evolve by a selective process different from those of natural and operant selection. That being the case, the second consequence is that the study of cultural selection would occur at a different level of analysis. Therefore, and here we have our third consequence, a new unit of analysis would be necessary. For Glenn (1988), "if the selection of cultural practices is to be considered a 'third kind of selection' . . ., we need to distinguish between the contingencies in the second kind of selection (behavioral contingencies) and the contingencies in the third kind of selection" (p. 167). Glenn and Malott (2004) were very clear about that position as well: "Are the scientific classifications of behavior analysis necessary to do cultural analysis? We think so. Are they sufficient? We think not" (p. 131).

However, those assumptions have further consequences. Being a new kind of phenomenon, related to a different process of selection, occurring at a different level of analysis, and possessing its own conceptual framework, cultural selection would also not be reducible to operant selection. In addition, the very explanatory model of cultural selection based on the concept of metacontingency would not be reducible to the explanatory model of behavioral selection based on the concept of contingency. As Houmanfar et al. (2010) wrote:

> By this definition, *emergent phenomena* will usually *require a different set of principles and theories* than the principles and theories used to describe and explain the lower-level phenomena. In other words, we suggest that there is a *qualitative and substantive difference* between the two levels whereby the higher-level phenomenon cannot be properly accounted for purely by relying on the accounts developed at the lower level of analysis. (p. 83, italics added)

This idea seems to be largely supported in the metacontingency literature (e.g., Delgado, 2012; Glenn, 1988, 2003, 2004; Glenn et al., 2016; Houmanfar et al., 2010; Houmanfar & Rodrigues, 2006; Krispin, 2016; Zilio, 2019b). Two premises, one ontological and the other epistemological, are present in this idea. The ontological premise goes as follows:

> *1. Ontological premise:* There is a new kind of phenomenon – a new kind of selection process associated with the emergence and evolution of cultural practices.

Four conjectures (not empirically corroborated yet) support the novel character of the ontological premise:

*1.1* Cultural practices (defined as a subset of interlocking behavioral contingencies or as the behaviors of individuals or of people in groups under the control of cultural contingencies, cf. Glenn et al., 2016) produce outcomes that would not be produced otherwise (the so-called "aggregate product").
*1.2* Aggregate products generate consequences responsible for selecting interlocking contingencies, not necessarily acting upon individual contingencies associated with the members that carry out cultural practices. Thus, "interlocking contingencies," understood as a unit, are what is "selected," not behaviors.
*1.3* That is so because cultural practices are an *emergent phenomenon*. They emerge from contingencies of selection, but are not *reducible* to those contingencies.
*1.4* Therefore, there is a *qualitative and substantive* difference between the phenomenon studied at the metacontingency level (cultural practices) and the phenomenon studied at the contingency level (behavioral practices).

By its turn, the epistemological premise follows directly from the ontological:

*2. Epistemological premise:* behavioral concepts and principles (as in the three-term contingency) do not explain the maintenance and selection of cultural practices. Therefore, a new conceptual framework is necessary.

However, before accepting those assumptions, we need to clarify some questions that were only marginally (if at all) discussed in the metacontingency literature that defend the emergence and irreducibility (e.g., Glenn, 2003; Houmanfar et al., 2010; Houmanfar & Rodrigues, 2006; Krispin, 2016; Sandaker, 2010):

1. What counts as an explanation? What does it mean to say that some conceptual framework (i.e., contingency) does not explain cultural selection?
2. What does it mean to say that some phenomenon is at a "different level of analysis" in relation to other phenomenon? What is the conception of "level of analysis"?
3. What does it mean to say that a phenomenon X emerges from a phenomenon Y? What is the concept of emergence (a technical term in philosophy) present in metacontingency literature?
4. What is the conception of reductionism in the metacontingency literature? What does it mean to say that X (X being a phenomenon or an explanation) is irreducible to Y (Y being another phenomenon or another explanation)?

As with emergence, reduction is a technical term in philosophy. There are numerous models of reduction in philosophy of science. One cannot simply say that something is irreducible and not go further. It is necessary to define what kind of "reduction" is not possible. Our goal here is to discuss such questions and, by doing so, to evaluate the pertinence of ontological and epistemological premises present in

the metacontingency literature. However, we will not present a thoroughgoing discussion of each of these topics. It would be difficult to do that in a single lifetime of research, let alone in a single paper. We do intend, however, to present clear definitions about such issues that will guide our subsequent analysis of the relation between contingency-based and metacontingency-based explanations. In sum, we hope the reader will be able to understand what we mean by explanation, emergence, and reduction.

## What Counts as Explanation?

What counts as an explanation? Because this discussion does not go beyond the domain of behavior analysis and radical behaviorism, no matter if we study metacontingency or contingency, it is fair to assume that the idea of what counts as an explanation is the same in those domains. Quoting Skinner (1957), to explain consists of answering "what conditions are relevant to the occurrence of the behavior – what are the variables of which it is a function?" (p. 10). As a form of verbal behavior, to explain something is to describe the variables responsible for the production of this something (Chiesa, 1994). Therefore, to assume that variables/elements of domain X (say, "contingencies of selection") do not explain the production of a phenomenon in domain Y (say "cultural selection") means that it is not possible to locate the variables responsible for the production of Y in domain X or that the conditions in domain X are not relevant to the occurrence of Y. How those conditions and variables are found will be discussed later with experimental examples from the metacontingency literature.

## Emergence and Level of Analysis

What does it mean to say that some phenomenon is at a "different level of analysis" when compared to another? There are at least three possible definitions of level of analysis; two of them are intrinsically related to the problem of emergence. However, let's start with the one not related to emergence (Chalmers, 2006; O'Connor & Wong, 2012).

### Weak Definition of Level and No Emergence

A weak definition of level of analysis takes into account the very relation between subject and object or, more precisely, the interaction between scientists and their objects of study. In this sense, to be at a different level of analysis simply means to be under the control of different stimuli (different "variables"). So the behavior analyst who studies respondent relations (such as "fear conditioning") is under the control of different variables in comparison to another behavior analyst who studies operant relations (such as "stimulus equivalence").

Why this is a weak definition? First, it is too general and permissive. By this definition, we could say that two behavior analysts are under a different level of analysis even if they are supposedly studying the same behavioral phenomena, as two persons are rarely controlled by exactly the same variables when doing science. Another way to put it is to say that stimuli are never the "same" (or that they do not have exactly the same function) for each person. Second, and this is more important here, this definition is insufficient to support the ontological premise present in metacontingency literature, according to which that we are dealing with a new "kind" of phenomenon related to cultural selection. Therefore, taking into account the present definition of levels of analyses, two behavior analysts studying different aspects of operant relations (i.e., different variables affecting a response) would be at different levels of analyses. Nevertheless, is this sufficient to assume that they are studying qualitatively different phenomena? Probably not. Therefore, given the metacontingency literature assumes the existence of a different phenomenon, this definition is not sufficient to support their claim. Let's go a step further.

### Intermediary Definition of Level and Weak Emergence

An intermediary definition of level of analysis is that different levels of analyses imply relations among phenomena, concepts, laws, events, and so on, occurring at different levels. We say, for instance, that water is a substance constituted by molecules of hydrogen and oxygen. But it is not the mere aggregate of such molecules. Those molecules need to be in a specific configuration (or organization) for the substance "water" to emerge. It is assumed that the very existence of water, as well as all of its characteristics, is entirely explained by its molecular properties. It is said that water is *nothing more or beyond* the specific organization of hydrogen and oxygen molecules.

Even though (and this is the important point) the emergent substance may present unexpected and unpredictable properties under the point of view of a purely molecular analysis (such as "liquidness," the quality of being liquid), those properties are *in principle* explainable by *a posteriori* analysis at a more basic level.

This is an "intermediary" definition because to accept the constituent relations among phenomena at different levels of analyses, as well as the possibility of unexpected or unpredictable properties at the emergent level, is insufficient to support the irreducibility argument present in metacontingency literature. To date, no clear evidence that phenomena deemed as "cultural" can only be adequately explained using metacontingency and related terminology has been presented (see Zilio, 2019b for a thorough review). The question remains: Can the emergent phenomena (cultural practices; the liquidness of water) be, in principle, explained by an analysis at a more basic level (behavior; molecular constitution of water)?

### Strong Definition of Level and Strong Emergence

The strong definition of level of analysis is associated with a strong conception of emergence. According to this notion, even though constituted by phenomena at a more basic level, emergent phenomena *are irreducible and not explainable* by analysis at this more basic level. In this sense, there is in fact the emergence of something "new."

The common example in philosophical literature is consciousness (cf. Chalmers, 2006). It is said that consciousness (whatever the definition of "consciousness" may be) is a physical phenomenon constituted by and emergent from activities of the nervous system. However, it is not possible to explain (even in principle) the properties of consciousness only by what is known (or will eventually be known) about the nervous system. It is said that consciousness is something qualitatively new; something not reducible to the activities of the nervous system. Another common example is life. Even though life is assumed to be a biological phenomenon, one could argue that it is not possible to reduce life to the biological components of a living organism. As a matter of fact, the whole idea of "emergence" played a crucial role in the debate between vitalism and mechanism in biology. Vitalists are strong emergentists regarding life (O'Connor & Wong, 2012).

In strong emergence, we find the requirement to support both ontological and epistemological premises about the metacontingency: Cultural selection is a new phenomenon (ontological premise) different from selection at the contingency level; therefore, it is not explainable through analysis at this more basic level (epistemological premise).

## Reductionism

Up to now, we have presented a possible definition of explanation according to which to explain something is to describe the variables responsible for its occurrence. To assume that variables/elements of contingencies of selection do not explain cultural selection means that it is not possible to locate the variables responsible for the production of the emergence and selection of cultural phenomena with a contingency analysis. We also discussed three definitions of level of analysis and emergence, which were (a) the *weak definition*, according to which to be at different levels of analysis simply means to be controlled by different variables; (b) the *intermediary definition* of levels or *weak emergence*, according to which X is constituted by Y, and there is novelty about X that may not be predicted *a priori* from Y, but can be, in principle, explained *a posteriori* by Y; and finally, (c) the *strong emergence*, according to which X is constituted by Y, and there is novelty about X that cannot be explained (even in principle) by Y, which means that at the same time X is emergent upon Y, it is also irreducible to Y. We saw that only strong emergence supports both ontological and epistemological premises related to the metacontingency. With those definitions in hand, it is time to discuss our last question. What does it mean to say that X (X being a phenomenon or an explanation) is irreducible to Y

(Y being another phenomenon or another explanation)? What counts as reductive explanation?

Reduction can be seen as a relation between phenomena or entities. This is called *ontological reduction*, as described by van Riel and Van Gulick (2019):

> The term "reduction" as used in philosophy expresses the idea that if an entity x reduces to an entity y then y is in a sense *prior to x*, is *more basic than x*, is such that *x fully depends upon* it or is *constituted by it*. Saying that x reduces to y typically implies that x is nothing more than y or nothing over and above y. (para. 1)

Reduction can also be seen as relations between theories or explanations at different levels: ". . . the philosophical notion of 'reduction' might be cashed out in terms of an explanatory relation. If x reduces to y, then it can in a relevantly strong sense be explained in terms of y" (van Riel & Van Gulick, 2019, para. 13).

Whether epistemological reduction entails ontological reduction or not is a pertinent (and very difficult) problem in philosophy of science (van Riel & Van Gulick, 2019). A positive answer means that if an emergent phenomenon at a superior level X is explainable at a more fundamental level Y by taking into account its constituent parts/properties/elements (i.e., weak emergence), then not only explanatory reduction would be possible, but the very existence of X would be reduced to Y. We are not going to discuss this problem here. We concur with Kim (2008) when he says that ". . . reductive explanation is often an achievable scientific goal whereas reduction is an overreaching metaphysical aspiration that is seldom, if ever, realized" (p. 95). Consequently, we will focus here only on the possibility of reductive explanation.

The classic (or standard) model of explanatory reduction was proposed by Nagel (1961). Nagel's model of reduction is known as inter-theoretical, for he understands reduction as an explanatory relation between two theories, one of them (the secondary theory) being derivable from the other (the primary theory). Nagel's reduction is based on the deductive-nomological model of scientific explanation, first proposed by Hempel and Oppenheim in 1948 (Hempel & Oppenheim, 1948; Oppenheim & Putnam, 1958), according to which explanation is a logical relation between an *explanandum* (the phenomenon being explained) and an *explanans* (the laws of which the phenomenon is derived from). The *deductive* element of this model is in the requirement that the *explanandum* must be a logical consequence (that is, to be deducible from) of the sentences constituting the *explanans*. The *nomological* element is in the requirement that at least one of the sentences of the *explanans* must be a *scientific law.*

Nagel's (1961) model of reduction, as well as the deductive-nomological model of explanation, do not seem to be compatible with radical behaviorism (Moore, 2008; Smith, 1986; Zuriff, 1985). "Laws" in behavior analysis are taken as general descriptions of orderly relations between dependent and independent variables (cf. Skinner, 1961). A radical behaviorist proposes a different conception of explanation that does not imply a derivative relation between laws and phenomena, but only the functional description of variables responsible for the

production of such phenomena. Explanations in behavior analysis are not validated by some kind of derivability criteria, but by how these descriptions effectively guide human actions upon the phenomena in question, making it possible that the production of such an event can be replicated by others (given the same or very similar conditions).

### Pragmatic Reductionism

The importance of presenting Nagel's (1961) model here is that, even with changes and adaptations, the models that followed Nagel's are at some level related to it. An important point, however, is that a growing number of philosophers propose models of reduction based solely on scientific practice. That is, they are not relying on logical or purely epistemological requirements, but they are trying to understand what is commonly seen as reductive explanation in science. In other words, they are studying the behavior of scientists. Bickle (2008a) describes this proposal:

> . . . to turn straight to the published experimental reports, here to neuroscience's primary experimental literature, with an eye to first doing purely descriptive metascience – to making explicit particular features of the science that typically remain implicit in the practices themselves, and burdened with as few prior metaphysical or normative epistemological convictions as we can be. The result of such an investigation would be a description of what the scientists are actually doing. (p. 15)

This proposal resembles Skinner's (1956, 1983) empirical epistemology. Nevertheless, by studying scientific practices of reduction (especially in the domain of biological sciences, Brigandt & Love, 2012) some authors (e.g., Bickle, 2003, 2006, 2008a; Craver, 2006, 2007; Kim, 2005, 2008; Schaffner, 1993, 2006) have elaborated models of reductive explanations. Now we will present a summary of characteristics that support reductive explanations in science according to this literature.

First, we need to start defining what counts as an *explanation.* As we said earlier, to explain is to describe the variables responsible for the production of the phenomenon under analysis. This definition (derived from Skinner's) is equivalent to the "manipulationist" conception of causality, as well as with what is called the "new mechanism" in biological sciences, which are two of the main theories of causality/explanation defended when analyzing the literature on reductive explanation (Zilio, 2019a). So we've established a common ground on what counts as explanation. Explanation is *the description of the variables responsible for the occurrence of the phenomenon under analysis.*

The second requirement for reductive explanation is *weak emergence*. It does not make sense to reduce explanations that take into account variables that are not related to each other in the sense of being composed or constituted by each other (Zilio, 2016). That is why the parallel commonly made in metacontingency literature (e.g., Glenn, 2004; Houmanfar et al., 2010; Houmanfar & Rodrigues, 2006) on the irreducibility of behavior analysis to neuroscience on one side, and metacontingency analysis to contingency analysis on the other side, does not make any sense.

A relation of constitution/composition is absent in the case of variables studied in behavior analysis and the variables studied in physiology. We are not just talking about the same thing at different levels of organization. As Zilio (2016) pointed out:

> It is not possible to reduce a behavioral concept or explanation to a neurophysiological concept or explanation. This endeavor does not make sense when we are dealing with two scientific domains that have different subject matters. On one side, we have behavior analysis, interested in the study of contingencies of selection (the relations between environmental events and the actions of organisms). On the other side, we have neuroscience, interested in the study of physiological mechanisms that mediate behavioral relations. Because behavior analysis and neuroscience focus on different variables of study, the events that set the occasion for scientists' verbal behavior related to behavioral and neurophysiological explanations are distinct. . . . Reduction does not make any sense because [behavior analytic and neuroscientific] explanations have different referents and it is not possible to derive one for another. . . How to reduce a particular operant response to a particular neurophysiological activity of motor cortex? How to reduce a specific consequence of reinforcement (an environmental event) to a specific activity of ventral tegmental area, a brain region assumed to be (at least in part) responsible for the process of reinforcement at the neural level through the modulation of the synaptic efficacy by the liberation of dopamine . . .? In sum, how to reduce contingencies of selection to neurophysiological events? (p. 167)

Reductive explanation only makes sense if we assume an organism-centered approach (hence mentalistic; Moore, 1981), according to which our subject matter is located between environmental events and the actions of the organism like in cognitive neuroscience (Baars & Gage, 2010), as shown in Fig. 1.

Regarding the cognition/mind and brain relation, it is assumed (mainly to avoid dualism) that "cognition" or "mind," no matter how one defines it, is constituted/composed by brain processes (McCauley & Bechtel, 2001) and, at the same time, it is also considered an emergent phenomenon. Bechtel (2008) named this assumption the "heuristic identity theory". Continuing with Zilio (2016):

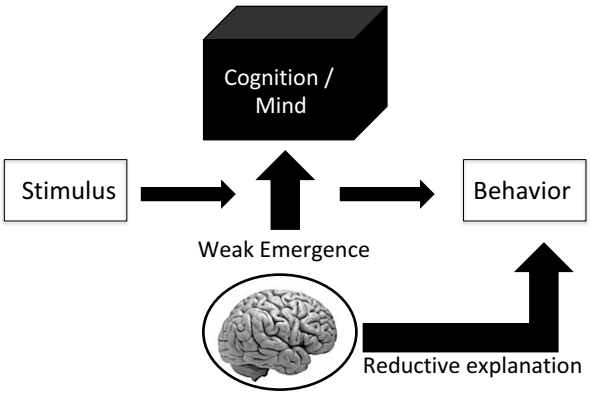


**Fig. 1** Reductionism in organism-centered (i.e., mentalistic) approaches

> One of the consequences of assuming the identity between cognition and brain is the possibility of reduction of cognitive processes or explanations to neurophysiological processes or explanations. There is an ontological thesis supporting the identity theory: Cognition *is* physical and it is somehow related to brain function. Reductionism is a valid endeavor in this case because there is a *synchronic* relation between cognition and brain. The main assumption is that something inside the organism mediates behavior (usually defined in cognitive literature only as observed effects of what happens inside the organism. . .). . . Synchronic relation can be defined as the coexistence *in time* of phenomena belonging to different levels of analysis. Briefly, at the same time, we have a brain process and a cognitive process occurring inside the organism, and both are supposed to cause behavior. (pp. 167–168)

However, that is clearly not the case with behavior analysis and neuroscience. In this case, there may be complementary explanations, with neuroscience providing information about brain mechanisms related to a given behavior and behavior analysis providing information about contingencies of selection that maintains such behavior. As Zilio (2016) concluded:

> In contrast, the relation between contingencies of selection and neurophysiological processes is *diachronic*. Diachronic relations occur *across* time. Therefore, brain events and behavioral events are not the same thing. Any attempt of reduction between behavior analysis and neuroscience is nonsensical. (p. 169)

Reductive explanation only makes sense in the context of emergent relations. Elements (variables, processes, phenomena, properties…) present in the reduced explanation must be constituted by elements (variables, processes, phenomena, properties…) present in the reductive explanation, which is the case of brain and cognition, as illustrated in Fig. 1. What makes this emergence "weak" is the fact that it does not matter if the emergent phenomenon (cognition or mind) presents unexpected and unpredictable properties, given those properties are in principle explainable by a reductive explanation (neuroscience) at a more basic level.

The third and last requirement related to pragmatic reductionism is that a reductive relation is not between two theories or explanations as in Nagel's (1961) model. Reductive explanation occurs when a phenomenon can be explained by taking into account processes occurring at more basic levels of constitution in comparison to

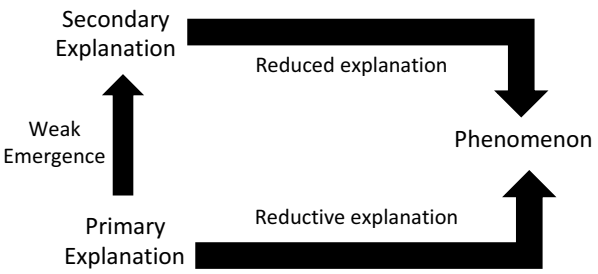


**Fig. 2** Explanation in pragmatic reductionism

explanations that take into account processes at superior levels of constitution, as shown in Fig. 2:

Reductive explanation occurs when a phenomenon can be explained (primary explanation) by taking into account processes occurring at more basic levels of constitution (hence the need for weak emergence) in comparison to explanations that take into account processes at superior levels of constitution (secondary explanation). We think this model of reductive explanation is appropriate to discuss the relation between contingency and metacontingency.

## Reductive Explanation and Cultural Practices

First, we have to describe exactly what phenomenon we are trying to explain. In scientific practice, reductive explanations are directed toward very specific phenomena (e.g., Bickle, 2007, 2008b). We are interested here in the *process of selection and maintenance of cultural practices*. We chose selection and maintenance of cultural practices because, according to metacontingency literature, this is the process supposedly not explainable by contingency analysis. Glenn (1988) defines cultural practices as "a subset of interlocking contingencies of reinforcement and a culture is made up of many such subsets" (p. 167). However, this does not seem to be a satisfactory definition for at least two reasons. First, the term "practice" indicates some kind of activity, the most probable in this context being behavior. It is odd to equate "contingencies" (interlocked or not) with "practices." How could something "practice" a contingency? How could a culture practice an "interlocking contingency?" Contingencies (cultural or not) are not "practiced"; they simply comprise the controlling conditions of behavior (cultural or not). Second, as Guerin (2001) pointed out, a considerable number of cultural behaviors do not seem to happen under the control of immediate interlocking contingencies. In other words, even if someone is behaving alone on a desert island, this behavior can be considered a "cultural practice" if it was selected by cultural contingencies in the past. For instance, a shipwreck victim ended up on this desert island and constructed a bamboo raft in order to get away. Although she is behaving alone, this behavior was learned through cultural contingencies: Someone may have directly taught her to make rafts, or she may have learned it while watching some adventure survivor reality show on television. Either way, constructing bamboo rafts can be considered a cultural practice. Based on those considerations, we adopt here the definition proposed by Fernandes et al. (2017): Cultural practices are behaviors of individuals or of people in groups under the control of cultural contingencies (that is, contingencies maintained by the members of a group or culture). This definition seems close to Glenn (2004), who defines cultural practices as "similar patterns of behavioral content, usually resulting from similarities in environments" (p. 140) and as "a class of acts that are functionally independent of one another" (p. 143). Therefore, cultural practices can be part of interlocked contingencies (given contingencies are composed of antecedent and consequent environment events and behaviors), but are not the contingencies per se. (Having defined what we mean by cultural practices, the appropriate thing to do would be to specify even more the *exact* cultural practice under analysis. We

will approach this with examples later.) With the phenomenon to be reductively explained defined, let's see if the other requirements for pragmatic reductionism are fulfilled.

The second requirement, as discussed earlier, is that the phenomenon or process at a higher level must be constituted by the phenomenon or process at the lower level. Glenn et al. (2016) defines metacontingency as "*a contingent relation between 1) recurring interlocking behavioral contingencies having an aggregate product and 2) selecting environmental events or conditions"* (p. 13, italics original). Therefore, metacontingencies are composed of interlocking behavioral contingencies of selection. As we saw in the beginning of this paper, it is also defended in the metacontingency literature that cultural practices are emergent from, but not reducible to, those contingencies. The second requirement for pragmatic reductionism is fulfilled by those assumptions.

Finally, is reductive explanation possible or at least conceivable in principle? In cases of weak emergence, reductive explanations are assumed to be possible in principle. To provide such explanations is an empirical endeavor – something that can be done only by scientific work. Figure 3 sums up the pragmatic reductive process we are formulating.

We have the phenomenon: selection and maintenance of cultural practices. We have a possible explanation based on the metacontingency, which is the explanation to be reduced. We also have the reductive explanation based solely on the concept of contingency. The question of reductive explanation can be approached in several ways. In the following sections we explore three: reductive interpretations, reductive thought experiments, and, finally, reductive explanation in experimental settings.

## Reductive Interpretations

The first approach is to provide interpretations (not explanations) of cultural practices based solely on the concept of contingency. To interpret is to give a possible understanding about a phenomenon not reachable (at least at present) through experimentation based upon knowledge produced in the experimental domain (Donahoe, 2004; Palmer, 2011). As Palmer (2011) wrote: "Many of nature's phenomena lie beyond our ability to measure, control, and observe, but science always interprets such phenomena in light of principles derived from observations made under optimal conditions" (p. 201). That is what Skinner did in his writings about culture and social phenomena. About two thirds of *Science and Human Behavior*

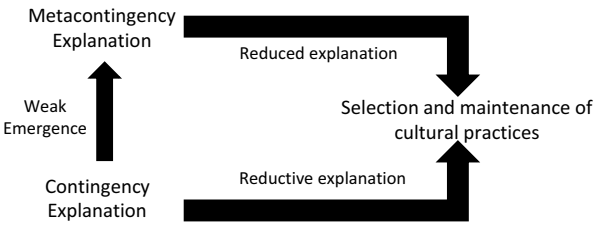


**Fig. 3** Reducing metacontingency explanations to contingency explanations

(Skinner, 1965) and the entirety of *Beyond Freedom and Dignity* (Skinner, 1971) are dedicated to interpretations of cultural and social phenomena based solely on contingency analysis. Therefore, we do not think it is necessary to explore this further here. Besides, as Zilio (2019b) argued, even though interpretations are very frequent in the metacontingency literature, they have not been based on principles derived from empirical data. In his words:

> Interpretations were made before any experiment on metacontingency and those experiments do not seem to provide a strong empirical base to support interpretations, as was the case with the operant. The trajectory was solely theoretical, from the proposal of the concept to its use in interpretations of social phenomena. It is different, for instance, from Skinner's interpretations of the social dynamics (Skinner, 1965; Skinner, 1971) or verbal behavior (Skinner, 1957), which were based on knowledge about the contingencies of selection gathered through the experimental analysis of behavior. As a consequence, one can argue this makes the possibility of interpretation using metacontingency a weak argument for its usefulness. After all, we can make "interpretations" of social phenomena using any model or theory not circumscribed by empirical data, as long as they are about the social. The sky is the limit. However, if one argues the empirical data is to be found in the experimental analysis of behavior, then the problem whether metacontingency is necessary arises again, since those data are about contingencies of selection and not metacontingencies. (Zilio, 2019b, p. 67)

### Reductive Thought Experiments

The second approach is to discuss hypothetical cases ("thought" experiments) of cultural practices and explain it only by contingency analysis. Actually, this is common practice in philosophy and in the sciences, especially physics (Stuart et al., 2018), and a significant part of the metacontingency literature uses this very approach to defend its proposal (Zilio, 2019b), particularly in the domain of organizations. Carrara and Zilio (2015) adopted this strategy with an example of a furniture manufacturer. In the following, we will describe an adapted version of this example. "Reductio" is a small company interested in manufacturing eco-friendly furniture. Five persons work at Reductio, each one having a particular and relevant role for the business: *Purchase Manager* – responsible for contacting suppliers, estimating prices and purchasing materials used in the manufacturing of the furniture; *Designer* – responsible for creating new furniture designs and models; *Craftsperson 1:* responsible for assembling the furniture based on the models created by the designer; *Craftsperson 2:* also responsible for assembling the furniture, but focuses mainly on the finishing; *Sales & Salary Manager:* responsible for researching the marked interest in buying the furniture, for selling the furniture to the stores, and for distributing the profits (salary) among the co-workers of Reductio.

Reductio is a "microculture" in the metacontingency framework of analysis (Glenn et al., 2016). The contingencies related to the behavior of its five employees

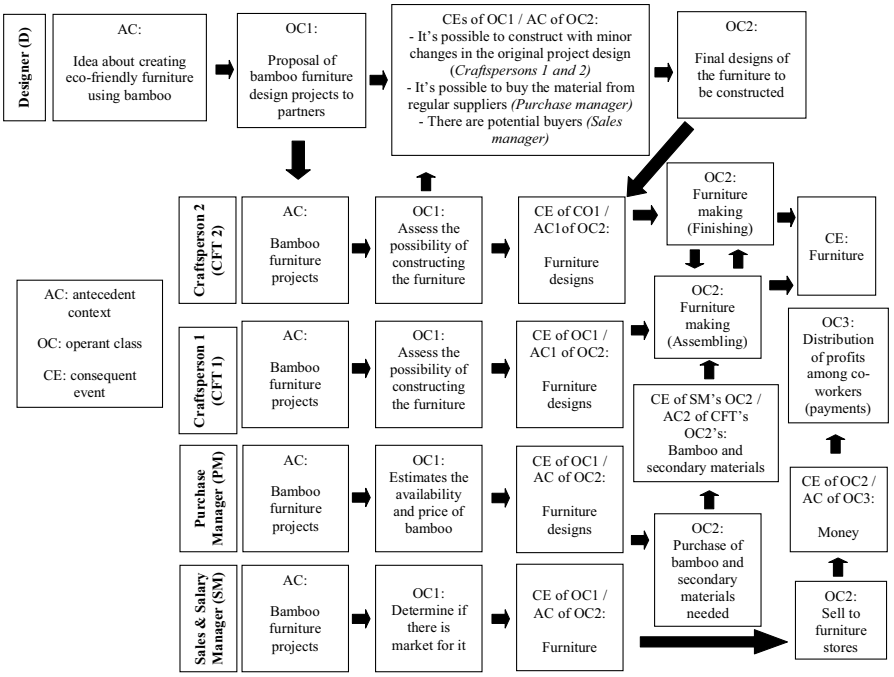


**Fig. 4** Explanatory reduction on Reductio (Adapted with permission from Carrara & Zilio, 2015)

are interlocked and those interlocked contingencies constitute cultural practices. The furniture is the "aggregate product" which could not be produced except by the interlocked contingencies that constitute Reductio as a microculture. The stores are the "receiving systems" and money the "cultural consequence" responsible for selecting the cultural practices that constitute Reductio. Figure 4 illustrates the dynamics of Reductio.

The designer decides to create a line of eco-friendly furniture using bamboo as the primary material. She presents the project to the purchase manager who estimates the availability and price of bamboo with the suppliers and returns to the designer saying that is possible to buy the material from regular suppliers for a competitive price. The designer also presents the project to the sales and salary manager who evaluates whether a potential market for this kind of furniture exists. She can present, for instance, a portfolio to potential buyers to see if they are interested in buying those models of furniture. Then the designer starts developing the actual models of furniture made of bamboo. Both craftspersons supervise the project by giving feedback about the possibility of constructing the furniture, that is, by saying if it is doable. Here we have interlocked contingencies that constitute the microculture Reductio. The furniture models made by the designer are part of the antecedent context related to the behaviors of the sales and salary manager, purchase manager, and craftspersons. Their feedback about the possibility of buying the material, constructing the furniture, and selling it are consequent events for the proposal made by the designer. The interlocking operant contingencies related to the behaviors of the

five workers associated with this company are all relevant to the production of the actual furniture (the so-called aggregate product). However, the furniture is not what maintains those five workers behaving in such ways: the financial return is paramount. So the sales and salary manager sell the furniture and distribute the profit among the five workers. The money earned from selling the furniture might not be the only reinforcing consequence related to the behaviors of those workers (to run an eco-friendly business might be another one); but in our hypothetical case, it is the primary consequence that maintains the practices of microculture Reductio. If the bamboo furniture is not selling, what is the point of continuing to make it? In sum, to present a very detailed interpretation of what is happening in Reductio without using the metacontingency is possible. The concept is not necessary to present a plausible (and more detailed) interpretation of the contingencies contributing to the maintenance of this microculture.

### Reductive Explanation in Experimental Settings

The last approach, and perhaps most important one, is empirical work. Can we explain the selection and maintenance of cultural practices (that is, describe the variables responsible for their production) by focusing only on contingencies of selection? Note this is not the same as focusing on the behavior of individual organisms. The focus is on interlocking contingencies and the consequences responsible for maintaining individual behaviors according to these contingencies. However, interlocking contingencies still are at the contingency level. There is not something qualitatively different; a different process of selection is not required. They are only contingencies in relation with each other.

Experimental studies that use metacontingency and derived concepts both for the basic design of the study and as a main conceptual tool for analysis have increased in number since the publication of Vichi et al.'s (2009) experiment and led to a prolific research field (e.g., Borba, Silva, et al., 2014a; Borba, Tourinho, & Glenn, 2014b, Borba et al., 2017; Cavalcanti et al., 2014; Costa et al., 2012; Morford & Cihon, 2013; Neves et al., 2012; Ortu et al., 2012; Pavanelli et al., 2014; Saconatto & Andery, 2013; Sampaio et al., 2013; Smith et al., 2011; Soares et al., 2012; Soares et al., 2018; Soares et al., 2019; Tadaiesky & Tourinho, 2012; Vichi et al., 2009; Ward et al., 2009), although not without its difficulties. As pointed out by Martins and Leite (2016), Velasco et al. (2012) and Zilio (2019b), this experimental research field still lacks methodological refinements needed to guarantee greater experimental control and, consequently, more robust data and replicability. In sum, all of these studies begin with a premise that they are investigating an emergent phenomenon, not reducible to a behavioral level of analysis, for which proper data analysis and discussion required a new theoretical tool (metacontingency).

It is also noteworthy that studies in the experimental analysis of behavior (EAB) using small groups of participants – usually called laboratory *microsocieties* or *microcultures* – are not new research strategies within the field (e.g., Azrin & Lindsley, 1956/1972; Brown & Rachlin, 1999; Emurian et al., 1978, 1985; Fantino & Kennelly, 2009; Hake et al., 1973; Schmitt, 1984, 1998, 2000; Skinner, 1962; Yi

& Rachlin, 2004; for further study on the history of the experimental analysis of social behavior, see Ulrich & Mountjoy, 1972, and, more specifically on cooperation, Marwell & Schmitt, 1975). These studies do not raise the question whether they are focusing on phenomena beyond a behavioral level of analysis and, thus, no new analytical unit is supposedly required.

Furthermore, collaboration between behavior analysts and researchers from other fields also led to fruitful results without needing metacontingencies and related concepts to explain the data (e.g., Baum et al., 2004). In an earlier work, Baum (2000) insisted that, despite the complexity of cultural phenomena, it still comprises and must be studied as behavior. Moreover, his recent theorizations on what he terms as *multi-scale behavior analysis* (Baum, 2017, 2018a, 2018b) could imply that his approach to studying culture leads him to treat these phenomena not as emergent from behavior at all, but as observation and measurement of behavior – and inter-related behavior between multiple individuals – from a broader time scale, which does not require a new unit of analysis or measurement. In his view, complex behavioral phenomena, such as emotions or cultural practices, demand time allocation data from a broader time scale.

That being said, three important questions must be asked regarding the experimental findings analyzed under the term metacontingency. First, what is being measured (the dependent variables; DV): social behavior, cultural practices or something else? Second, what is being manipulated (independent variables; IV) to assess its effects on the DV. And third, is it possible to analyze the interrelations between DVs and IVs of experiments from the area of cultural analysis satisfactorily and without leaving gaps that supposedly could only be explained by metacontingencies? This last question is at the core of the pragmatic reductionism proposal. In the remainder of this article, we give a positive response by re-interpreting data from metacontingency experiments without using the concept.

Five experimental studies were selected for this exercise, each one for particular reasons and relevance to the field. The first is Vichi et al. (2009), which was selected because it has been deemed the first metacontingency experiment and, therefore, the first demonstration of cultural practices selected by cultural consequences. The second is a paper containing five small experiments conducted by Ortu et al. (2012) using the Iterated Prisoner's Dilemma Game adapted to fit a metacontingent description of cultural selection. This paper was selected because experiments based on social dilemma games are common in behavior analysis and in other fields for the purpose of studying social phenomena; thus, its inclusion seemed relevant. The third study, Toledo et al. (2015), was selected because it adopted a procedure that does not use discrete trials, like all the other procedures employed in metacontingency experiments. Their procedure is arranged as what they call a "free culturant" (the term "culturant" was proposed by Hunter (2012) as the unit selected in metacontingencies, placing the term as an analog for the *operant* on a third level of selection), marking a parallel with free operant procedures. The fourth study was conducted by Velasco et al. (2017), and was selected because it was the first to use non-human subjects (pigeons) to assess the possibility of the emergence of metacontingencies; all other metacontingency experiments have dealt with human subjects (and almost exclusively

undergraduate students). And finally, the fifth is a study by Soares et al. (2018) in which verbal and non-verbal consequences were manipulated. One strength of this study in comparison to the others selected is that it explores the role of verbal consequences which is of particular relevance to the analysis of culture, and, for that reason, it was also selected for this exercise.

### Vichi et al. (2009)

Vichi et al. (2009) verified the effects of applying cultural consequences on the cultural practice of resource allocation among group members. They adapted an experimental task used by Wiggins (1969), a study from the field of experimental sociology that was not developed by a logic concerning metacontingencies. During Vichi and colleagues' experiment, a complete trial (or "round" as they called it) would have the following steps: (a) each individual of a four-participant group was required to bet three to ten tokens which were aggregated as a "group bet"; (b) they were asked to discuss and collectively choose a line numbered 1 to 8 on as 8x8 colored matrix presented to them; (c) the experimenter would choose a column by stating its color; (d) the experimenter announced a plus (+) or (−) sign in the intersection cell, (e) the participants would receive their payoff on the "group bet," dependent on the experimental condition in effect (described below); and (f) the participants distributed the tokens earned in that round among themselves (these steps are presented in Fig. 5).

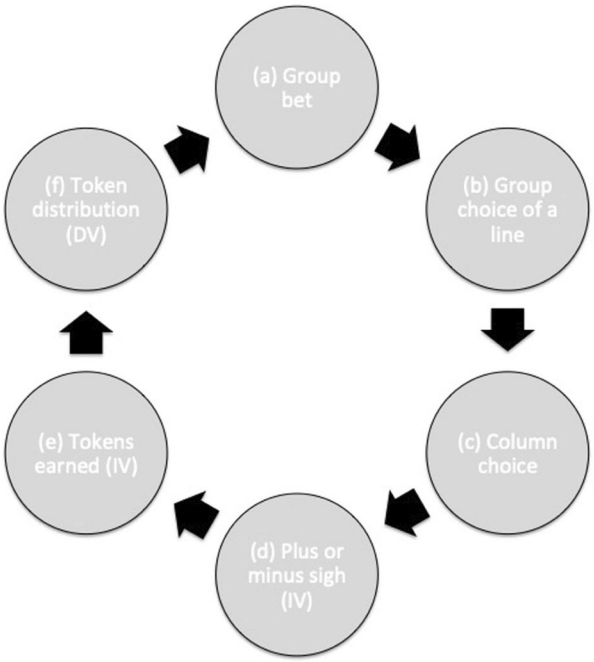


**Fig. 5** Flow of events in a round in Vichi et al.'s (2009) experiment

The DV in Vichi et al. (2009) was resource allocation, that is, how participants distributed their tokens in step (f) of each round. In experimental condition A, the experimenter would choose columns which resulted in plus signs – doubling the number of tokens in the "group bet" bowl – whenever the participants equally distributed their tokens among themselves. Whenever they made an unequal distribution, the number of tokens in the bowl would be cut in half. In condition B these requirements were inversed, favoring unequal token allocation. Thus, the independent variables, the cultural consequences, can be considered the plus or minus sign presented in step (d) or the tokens collectively received in (e). However, the experiment has a "catchy" aspect: because the DV (resource allocation cultural practice) only occurred in step (f) of a trial, the supposedly "cultural reinforcing" events happened before steps (d) and (e). To deal with this complication, the plus sign was presented contingent on how participants distributed their tokens *on the previous trial.* For example, say that in trial 23 they fulfilled the criterion established by condition A (equal distribution). Given this event (token distribution), they would access both the plus sign, as well as the higher amount of earned tokens in trial 24. This meant that group consequences were presented temporally distant from the cultural practice it was intended to affect, but also from events (a), (b), and (c) as for presenting the plus sign consequence and adding (d) before accessing effects of the token consequences on (e). Also noteworthy is that between the DV and presentation of IVs another round would be initiated. Figure 5 presents the flow of events in each round. The experimental programming resulted in a study that aimed at evaluating the selection of a cultural practice by a *consequence*, which, considering discrete trial procedures, as used here, should be presented *after* the DV and in the same trial.

Despite Vichi et al.' (2009) interpretation of their results (i.e., that selection may have occurred at a different level of selection captured by the concept of metacontingency), another interpretation is possible: the programmed cultural consequence may have varied in frequency due to operant consequences in the different steps in each round or due to extra-experimental events not assessed. A strategy the experimenters used to force occurrence of the target cultural practice was to intervene right before the participants distributed their tokens, such that participants retained a number of tokens that were divisible among themselves only according to the experimental conditions in place (equal distributions for condition A and unequal for condition B). The order of exposure to the experimental conditions for group 1 was ABAB and group 1 quickly achieved the criteria to switch to condition B, in which they spent a large number of trials and 14 experimenter interventions were needed before unequal distributions occurred stably and the criteria for the condition switch was again achieved. Group 2 began with condition B and 24 interventions were needed before reaching criteria.

The information above puts more doubt on the claims that token distribution went through cultural selection as a function of the programmed cultural consequence. Another possible hypothesis is that more optimal results for condition A could indicate conformity, in the sense that, in that particular social context, participating in a research activity in front of a researcher and having to collaborate with other people, they tended to act in a more "socially acceptable" fashion (Axelrod & Hamilton, 1981; Fehr & Gächter, 2002), which made imitating others more

likely. When in direct contact with one another, and with the possibility of verbal interactions (which also implies the verbal behavior of one individual may acquire the function of social reinforcement for another's behavior), participants tend to behave prosocially (Borba et al., 2014a, b; Soares et al., 2018).

According to the arguments presented above, Vichi et al.' (2009) results could be interpreted in terms that do not include metacontingency selection. It can be argued that equal token distribution (in condition A) occurred as a function of the social context in which they executed a group task with strangers, which increases the probability of conformity to social norms shared by the group on how to behave in similar situations, as well as a function of in-group imitation. As for condition B, unequal token distribution occurred stably only after several experimenter interventions, which may imply that unequal resource allocation was simply imitation behavior emitted by the participants under control of the experimenter's behavior; that is, operant contigencies (double/half the previous bets) modified the behavior of individual who was doing the resource allocation, which is not necessarily a product of cultural selection. The fact that in both cases, conditions A and B, each participant received higher individual payoffs for behaving in accordance with the target cultural practice (that could be understood as a performance requirement of a contingency of reinforcement) could mean that the programmed consequences simply resulted in the production of a reinforcer of greater magnitude for each participant. With this interpretation, the study conducted by Vichi et al. (2009) does not support a clear conclusion that cultural practices were selected by a cultural consequence. Although token distribution and betting patterns became recurrent, it cannot be affirmed that these were products of cultural selection. The data obtained can be explained without referring to metacontingencies and related concepts.

The fact that Vichi et al. (2009) was considered the first experiment to provide empirical evidence supporting the concept of the metacontingency highlights the importance of making an effort to interpret the data using well established conceptual tools before shifting to new theoretical proposals. That effort would enable the authors to rule out these possible alternative explanations first, and only then propose a newer and, back then, only theoretically explored concept to explain their data. By first thoroughly ruling out alternative operant explanations, the strength of the metacontingency as a valuable analytical tool for the events explored in the experiment would be more evident. However, this effort is absent not only in Vichi et al. (2009) but in metacontingency experimental literature as a whole (Zilio, 2019b).

### Ortu et al. (2012)

Ortu et al. (2012) conducted a series of five experiments using an Iterated Prisoner's Dilemma Game (IPDG) – a variant of the Prisoner's Dilemma Game (PDG) including repeated trials – as their basic procedure, making adjustments in order to fit a metacontingency-oriented analysis. The PDG is one of many social dilemma games – simulations to assess the effects of social variables on behavior, mainly on decision-making – developed from Game Theory (Bierman & Fernandez, 2011; Bolton & Ockenfels, 2000; Meyer, 2010; Rachlin, 2002). In a PDG, participants are put in

a decision-making situation in which they can choose to "cooperate" or "defect." If we consider the generalized reinforcer applied as tokens, those who cooperate earn ($n$ x 3) tokens and those who defect receive ($n$ x 3) + 7 tokens, $n$ being the number of players that choose to cooperate. This arrangement implies that the highest equal earnings a group of participants can receive – considering a four-player group – would be when all participants cooperate. However, the highest amount an individual player could earn would be if all others cooperate and she is the only one who defects.

The PDG and its variants have been shown to be reliable research strategies to study social behavior within EAB, predominately analyzing and explaining the data based on the literature on cooperation and operant behavior (e.g., Brown & Rachlin, 1999; Fantino et al., 2006; Green et al., 1995; Meyer, 2010; Schmitt, 1976; Yi & Rachlin, 2004). Also, the PDG has influenced applied work within behavior analysis (see Fidelis & Faleiros, 2019 for a review). The use of an operant vocabulary regarding social behavior and cooperation seemed sufficient to explain data on recurrence of cooperative patterns in such studies.

Ortu et al. (2012) ran five experiments; in each, a different group of four participants took part in an altered version of the IPDG. In each cycle, each participant had to type the X key (cooperate) or the Y key (defect) on a keyboard and key pressing would produce a sum of money (in cents), following the rules of the PDG. To simulate a metacontingency arrangement, "market feedback," manipulated to serve as a cultural consequence, was contingent on the occurrence of an aggregate product, XXXX (all participants cooperating) or YYYY (all participants defecting), depending on the experimental condition in effect. The market feedback could be negative (loss of money), contingent on three of the five possible aggregate products, or neutral (no gain or loss of money), or positive (earning money) for the other two aggregate product combinations. Thus, the supposedly programmed metacontingency would be the contingent relation between the player interactions (interlocking behavioral contingencies [IBCs]; not specified or measured), the key combination resulting from the choices of all four participants (the aggregate product; its occurrence measured by the software used) and the market feedback (cultural consequence).

Experiment 1 basically tested the procedure of applying a market feedback contingent on coordinated responding by the participants, shifting between conditions XXXX and YYYY throughout the study. Eventually both patterns occurred stably, although with some difficulty (especially with YYYY), given it was necessary to apply what they called "feedback maximum" (Ortu et al., 2012, p. 117) or "maximum adjustments" (p. 118), that is, increasing the magnitude of market feedback (or cultural consequence). Experiment 2 systematically replicated Experiment 1, adding a baseline condition and applying lower values of feedback maximum. Experiment 3 differed from Experiment 2 by eliminating feedback maximum adjustments. The results show reliable production of XXXX and YYYY when market feedback was applied, in comparison to the baseline condition. Experiment 4 applied non-contingent market feedback, with this consequence delivered on the same cycles as in Experiment 3, regardless of the participants' responses. Ortu et al. observed a high increase in the occurrence of XXXY and XXYY patterns during the non-contingent

market feedback conditions. Finally, in Experiment 5, more reversals were done with progressively lower market feedback across reversals and Ortu et al. observed that even when participants lost money (last YYYY condition), the target pattern still occurred stably.

The first point that should be noticed is the confusion made by Ortu et al. (2012) when they state that the "independent variable was the metacontingency" (p. 114). This is a categorical error. If the term metacontingency refers to inter-relations among IBCs, an aggregate product, and a cultural consequence, as it does in metacontingency literature, what Ortu et al. (2012) objectively manipulated was the delivery or non-delivery of market feedback (the cultural consequence) and changes in its value, observing its effects on the occurrence of two different response combinations (aggregate products XXXX and YYYY) as the DV. A metacontingency, like an operant contingency, is not a discrete event that can be manipulated and its effects on another event assessed. It is a verbal description of an interrelation between events; it is a conceptual tool used to interpret the data about what happens to one event when another interrelated to it is manipulated.

On the claim that the data presented support the hypothesis that a metacontingency was successfully observed, other factors, as candidates for alternative explanations, need to be evaluated. First, it is possible that the market feedback increased the individual earnings and that alone could explain the higher rate of all players pressing X or Y. The demand that all players should press the same key could be thought of simply as a refinement in stimulus control, establishing the contingency that "when in the game with others, if X, then higher earnings." The stability of the occurrence is likely to be due to players talking about events in the game and verbally describing such a contingency to one another (e.g., they could use a chat tool to write to each other during the experiment), establishing rule-following as contingent to earning the market feedback. This is even more likely in condition YYYY because, although pressing Y when all others did so too also resulted in money loss with regular IPDG rules, market feedback inverted it so it resulted in earning money.

Maintenance of such responding in further conditions even when YYYY resulted in money loss could be due to rule-governance, with rule-following strengthened by constant social reinforcement and/or punishment in the group situation participants were exposed to in the study. Also, verbal descriptions regarding the YYYY combination could be understood as symbolic (i.e., socially attributed) properties attributed to stimuli that can exert consequential control on participants' behavior (Foxall, 1999, 2001). Even though analysis of verbal interactions during the experiments would be needed to support these claims, these hypotheses are based on assumptions sustained by well-established data in behavior-analytic literature. Thus, this should be the first as possible explanation considered rather than assuming that a non-established theoretical construct is needed instead. Therefore, to support Ortu and colleagues' explanation of the data obtained, they needed to first analyze the verbal data to rule out alternative explanations. As in the case of Vichi et al. (2009), Ortu et al. did not make an effort to rule out alternative explanations grounded on better data and empirically supported concepts, a much-needed endeavor before using a conceptual tool with very little – and controversial – experimental support.

The justification for not needing to analyze verbal interactions Ortu et al. (2012) provided at the end of the article – "It is not clear that such explanation is required any more than explanation of neural changes is required to establish the functional relations of operant analysis" (p. 120) – is not applicable. We already illustrated the problems of reductive thinking in neuroscientific and behavior analytic explanations, on one side, and explanations based on contingency and metacontingency on the other. The first does not make sense as it is clearly not conceptually acceptable. The second, however, is a legitimate and paramount endeavor. In addition, while the use of operant analysis alone to explain behavior has a robust body of reliable data supporting it, metacontingency analysis of cultural phenomena is mainly supported by theorization or problematic experiments (Zilio, 2019b). Therefore, operant and metacontingency explanations are far from equivalent regarding the data to support them in order to enable Ortu and colleagues' justification to be sufficient for ignoring the analysis of participants' verbal interactions.

### Toledo et al. (2015)

Toledo et al. (2015) presented a software called *Free Culturant*, that "allows moment-to-moment measurement of the frequency of both individual behavior and interrelated behaviors" (p. 370), which they argue had not been possible before in the experimental analysis of cultural phenomena. They describe several features of the software, but for the purpose of the current analysis, we will focus on the general properties of the experimental task: programming contingencies and programming metacontingencies. Toledo et al. also present a pilot study conducted using their software, which will also be considered here.

The experimental task featured allows a maximum of four participants and consists of a "hypothetical situation, in which residents of a small city have gone several days without water, and the participants can contribute by filling water jugs for them" (Toledo et al., 2015, p. 370). Participants can do this by using the mouse cursor and clicking at the on-screen water tap (manipulandum). Each participant had a water jug to fill.

Toledo et al. (2015) also mention that contingencies and metacontingencies can be programmed. For operant contingencies, different schedules of reinforcement (CRF, FR, VR, FI, VI, FT, and VT) can be applied to affect the contingent relation between the water tap pressing response and the product of the amount of water in the jug, which is the reinforcing stimulus. It is also possible to suspend water production despite water tap responses, enabling an extinction procedure.

The programming of metacontingencies proposed in Toledo and colleagues' procedure demands coordinated responding by the participants which, when it meets a specific criterion, fills a portion (called a bonus) of a water tank located on the top of the screen, common to all participants' interface. The possibilities of coordination required are based on procedures used in studies of cooperation (Cohen & Lindsley, 1964; Marwell & Schmitt, 1975; Schmitt & Marwell, 1968) in which each participant, responding on their own manipulandum, must coordinate their responding according to a temporal dimension in order to produce reinforcement.

The reinforcement requirement could be of temporal proximity (e.g., both responding within and interval of 0.5 s from one another), or with a specific delay (e.g., the second participant responding within an interval of 3–3.5 s after the first). Here, water tank filling (deemed as a cultural consequence) could be programmed to be contingent on the first ("same time" or 0.5 s interval) or the second ("spaced" or within intervals up to 3.5 s) cases presented above; to a specific order of clicking by the participants; to FT or VT schedules; and could not be presented at all, simulating extinction.

In the pilot experiment, six participants divided into two triads were exposed to different experimental designs. In Triad 1, VR2 schedules were programmed as operant contingencies, with each water jug filled rendering R$ 0.01 ($0.003 approximately) for the participant at the end of a session. Metacontingencies varied along conditions. At first, in baseline, only operant contingencies were applied. This was followed by a spaced 1s condition, then a return to baseline. The following four conditions were spaced 1s, 2s, 1.5s and 2s, respectively, as the criterion to produce the bonus (fill a portion of the common water tank). Each bonus could be exchanged for R$ 0.10 ($0.03 approximately) at the end of a session, and the participants could divide this amount among themselves however they wanted.

The programmed contingency and metacontingencies implied a conflict. In a VR2 schedule, high response rates produced a greater number of reinforcers, while the spaced criterion for bonus production implied that response rates had to be lower. Thus, producing more bonuses resulted in less reinforcement for each participant according to the programmed operant contingency. The results show a low coordination rate in baselines, a sharp increase in conditions of 1s and 1.5s, and no occurrence in the 2s conditions. Response rates are high in baselines. They decreased in conditions of 1s and 1.5s and increased in those of 2s.

Triad 2 was exposed to a no-conflict design in which the operant contingency was a VI 10s schedule and conditions regarding bonus productions were baseline, spaced 1s, 1.5s, 2s, and baseline, in that order. Response rates were high at the beginning of baseline then lowered progressively, and remained stably low throughout all other conditions. Coordinated responses began to occur by the end of baseline and increased stably throughout conditions 1s, 1.5s, and 2s, with very few occurrences at the final baseline.

The authors interpret their data as evidence that a cultural unit is selected by the supposed cultural consequence applied. However, as in Vichi et al. (2009) and Ortu et al. (2012), Toledo et al. (2015) do not make an effort to analyze their data with concurrent operant explanatory alternatives. Given the time frame in which these three studies were conducted, they may be generating a cumulative effect of overconfidence on metacontingency explanations, as the researchers did not consider alternative explanations of their data. This is quite problematic for the development of the field not only because it may decrease the variability of research programs in culturo-behavioral science but, most important, because it means allocating our scientific efforts on an explanatory model that seems to be completely unnecessary. Again, the cooperative response obtained by Toledo et al. could be seen through operant contingencies, viewing the requirement to respond in a specific coordinated manner with other participants as a refinement of stimulus control. This could have

been verbally described and, in doing so, the participants' performance could be closer to accurate, meaning that cooperative responding here could again be viewed as rule-governed behavior. Similar to the argument presented regarding Ortu et al. (2012): by not analyzing verbal interactions, the authors do not give an opportunity for an explanation based on rule-governance to be considered.

Another important consideration is the high difference in magnitude for the payoff for cooperative and individual responses. One cooperative response produced one bonus (one portion of the water tank) at a value of R$ 0.10 ($0.03 approximately). In comparison, the small R$ 0.01 ($0.003 approximately) consequence was contingent on filling a whole water jug, which required several portions of water filling consequences. This meant that cooperative responses produced higher magnitude reinforcers, making them a preferable choice, especially if one considers explanations regarding the effects of concurrent schedules on choice behavior based on the matching law (Herrnstein, 1961).

Considering both hypotheses presented above, which could be combined for an even stronger alternative explanation, the changes observed in response rates and the occurrence of cooperative behavior in Triad 1 could have been due to applying and retracting a situation with concurrent schedules, with cooperative responding resulting in higher reinforcer magnitude. In Triad 2, schedules for both individual and cooperative responses were interval-based, the first being VI 10s. Cooperative responses could have a maximum interval of 6s (three participants responding in 2s spacing condition). This meant that they not only produced higher magnitude reinforcers (as described above), but their production also required less waiting time between responses, making cooperative responding even more attractive than in the case of Triad 1.

Therefore, again, Toledo et al. (2015) is a case in which overconfidence in one theoretical assumption may have led the researchers not to consider alternative explanations based on better established conceptual tools with more data that supports them.

#### Velasco et al. (2017)

Velasco et al. (2017) did a cooperation experiment with pigeons and analyzed their data using the metacontingency. Therefore, there was not a possibility for verbal behavior. Taking the authors' own review and descriptions of cooperation experiments, usually contingencies are programmed so that either reinforcement for both organisms – mutual reinforcement (cf. Tan & Hackenberg, 2016) – is produced by coordinated responding (e.g., Azrin & Lindsley, 1956; Cohen & Lindsley, 1964; Skinner, 1962); or concurrent schedules are programmed so that the organisms must choose between emitting an individual response (producing reinforcement only for itself) or the cooperative response (producing mutual reinforcement) (e.g., Marwell & Schmitt, 1975; Schmitt & Marwell, 1968). However, as put by Velasco et al., "demands for coordination in metacontingency studies … are usually compatible with demands for what the participants do individually (i.e., participants' responses can produce individual and mutual consequences simultaneously)" (p. 3). Two points that should be highlighted thus far are that (a) the authors do not question

the operant analyses on the cooperation experiments described, and (b) they differentiate metacontingency experiments from cooperation experiments and develop a procedure in which the only increase in complexity is the compatibility between contingencies that produce individual and mutual reinforcement. This raises a natural question of whether this slight procedural difference in the experimental arrangements really required a new unit of analysis in order to make sense of the data. Even with this experimental enhancement in complexity, could their data be suitably understood by operant analyses of cooperation contingencies?

Velasco et al. (2017) used six naïve adult pigeons (*Columba livia*) and a custom-built operant conditioning chamber divided into two equal spaces with a transparent wall or opaque barrier, thus controlling whether each pigeon could see the pigeon in the other compartment. Each compartment contained two translucent response keys, horizontally aligned, that could be illuminated with a green, red, or white backlight. The pigeons had to stand in front of each other to peck the keys. Opposite to the transparent wall was a third response key, illuminated by a white light only, and a food tray.

The pigeons first passed through preliminary individual training to establish key-pecking. Afterwards, they were distributed into pairs and exposed to four experimental conditions, with one pigeon of each pair placed in each compartment of the operant chamber. In the first phase, only the individual contingency was applied, with 3s access to food as the reinforcer. The second phase applied both individual and mutual contingencies, with 4s access to food as the mutual consequence. Notice that if demands for both the individual and mutual contingencies were met, then both consequences were produced, with a total of 7s access to food. When the keys were lit red, the coordinated behaviors required were for both pigeons to peck parallel keys within an interval of ≤5s from one another. When the keys were lit green, the coordinated behaviors required were for both pigeons to peck diagonally aligned keys within an interval of ≤5s interval from one another. In the Position Interchange Phase, which began after 70 sessions, the pair of pigeons switched positions in the chamber every 10 sessions to rule out spatial control over responding. Finally, at the Interchange Among Pairs Phase, subjects changed pairs, permitting testing as to whether their responding would generalize toward different pigeons.

The results presented by Velasco et al. (2017) do not show consistency in the increase in frequency of the target-coordinated responses resulting from the application of mutual consequences. For S1–S2, mutual consequences did not affect coordinated responses and resulted in a reduction in the frequency of individual responses. Here the compatibility to produce both individual and mutual consequences could have acted as a confounding element for the pigeons, making it more difficult to select cooperative responding.

As for pair S3–S4, S3 kept pecking the same key regardless of color and S4 switched keys according to lighting. This resulted in uneven consequences for each subject (4s of food for the first and 7s for the latter). Although Velasco et al. (2017) emphasize that these responses were "socially mediated" (p. 6), this is true only in the sense that the programmed contingencies demanded specific responding from *both* subjects in order to produce mutual reinforcement. However, it cannot be said that *coordinated responding* (i.e., cooperation) was selected, given that neither

pigeon's responding was under the stimulus control of the other pigeon's behavior. The production of mutual consequences seemed to happen by coincidence, i.e., from S3 always responding the same way and S4 alternating between keys. Therefore, here too the procedure employed could not be said to be effective to produce cooperation and is distant from a metacontingency analysis.

Finally, the results for S5–S6 at first showed strengthening of coordinated responses, which subsequently decreased by the sixth 10-session block, indicating inconsistent selection of coordinated responses by mutual reinforcement. During the Position Interchange Phase the subjects' responding followed similar patterns as that of S3–S4; S6 mostly responded on only one key, decreasing reinforcement for individual responses, but S5 continued to alternate between keys according to the corresponding color, resulting in frequent individual and mutual reinforcement. Therefore, mutual reinforcement occurred frequently, but it cannot be assumed that responses were actually *coordinated*; rather, they were most likely coincidental due to S6 not varying responses and S5 alternating responses. As for S3–S4, their responses seemingly were not under stimulus control of the other's behavior.

The results presented by Velasco et al. (2017) indicate that the increase in complexity, when compared to more traditional cooperation experiments (making individual and mutual reinforcement compatible), could have acted as a confounding element that compromised the subjects' performance. The authors' operant analyses of their data, mainly regarding choice behavior understood through the matching equation, seem sufficient to make sense of their results. Velasco et al. state their study "represents an important step forward" (p. 7) in integrating operant analyses of cooperation and cultural analyses from a metacontingency framework, but the contribution of the latter is seemingly null. At most, it inspired the application of compatibility of individual and mutual consequences, which in actuality served as a negative aspect of their procedure, as it most likely could be held responsible for the inconsistent results. When applying such complex social situations to non-verbal subjects, it seems that the subjects could not perform the task adequately, which raises more attention to the lack of analysis of verbal behavior in the studies already presented (i.e., Ortu et al., 2012; Toledo et al., 2015; Vichi et al., 2009), that if included could provide a more adequate explanation of their data without appealing to a conceptually fragile and non-empirically supported theoretical construct such as the metacontingency.

#### Soares et al. (2018)

Soares et al. (2018) used a variation of the "matrix task" employed by Vichi et al. (2009) that had been developed in the previous years (Cavalcanti et al., 2014; Gomes & Tourinho, 2016; Hosoya & Tourinho, 2016; Marques & Tourinho, 2015; Pavanelli et al., 2014; Soares et al., 2012). More specifically, they adopted a variation designed by Borba et al. (2017), in which programmed contingencies and metacontingencies are applied as concurrent schedules in order to simulate conflicts between individual and group consequences. Specifically focusing on the effects of consequential verbal stimuli on cultural selection, the authors refer to *verbal cultural consequences (VCC)* as verbal stimuli employed as reinforcers or punishers, termed

by them as VCC of approval or disapproval, respectively. They compared the effects of VCC with non-verbal consequential stimuli deemed as *non-verbal cultural consequences (NVCC)*. These effects were evaluated in conditions of competition and non-competition between individual and group consequences and variations in the order of exposure to VCC and NVCC.

Distributed among four experimental microcultures, 123 college students took part in this study (32 to M1, 29 to M2, 33 to M3, and 29 to M4). M1 (competition) and M2 (no competition) went through conditions following the order of ABABCD, and M3 (competition) and M4 (no competition) were exposed to BABACD, in which condition A employed NVCCs + VCCs, condition B applied only NVCCs, C used VCCs, and both NVCCs and VCCs were suspended in condition D.

Regarding the manipulation of the application of the VCC and/or the NVCC, the experimental design could be characterized as a single-subject design (considering each microculture a "subject"). Therefore, the procedures, as applied to a few subjects and complimented by assessing a different order of exposure to the experimental conditions, is not a problem in the evaluation of the effects of the variables manipulated given the results could be compared to the baseline data for the microculture. However, "competition/no competition" between operant contingencies and metacontingencies was applied only to different microcultures; neither microculture went through both conditions of competition and no competition, as would be the case in a single-subject design. This implies that this manipulation is one of group design, which requires a larger sample in order to thoroughly assess the procedure's effects, just to point out one aspect needed to adjust the procedure to proper methodological directives (Cozby, 2003). Therefore, as reported by Soares et al. (2018), the effects in conditions with and without competition between operant contingencies and metacontingencies could not be adequately assessed. Despite this problem, it can be inferred that the better performances observed in conditions of "no competition" could be due to a context of lower complexity (in terms of fewer variables manipulated) than in conditions with "competition." That is, in one case concurrent schedules were applied, and therefore, responding was allocated among two different choices, while in the other case concurrent schedules were not in effect.

Experimental conditions would shift either by a performance criterion (80% of target culturants in a 60-cycle interval; criterion A), or after 100 cycles (criterion B). One cycle comprised each participant making their choice and the contingent consequences being delivered. The results presented indicate that NVCC + VCC had stronger effects on the recurrence of target behavioral coordination among participants than when these consequences were applied in isolation. Two aspects of the procedures need to be highlighted here. First, the consequences were applied using different sets of operations. NVCCs were only delivered or not delivered, emulating a reinforcement procedure and an extinction procedure, respectively. Second, VCCs were of approval or disapproval (i.e., used for a reinforcing effect and punishing effect, respectively). These differences can lead to the different effects observed. Moreover, when combined, their effects could be thought of as being "summed" with one another, explaining the stronger effects.

Also, while the NVCCs can better be seen as group consequences, it is unclear whether the effects of VCCs could be assessed as group or individual. This is even pointed out by Soares et al. (2018) who state that "verbal consequences of approval and disapproval, applied individually, should also have their effects examined" (p. 44). As in the other studies analyzed here, the data presented seem to be explainable within the operant framework – mainly regarding findings on verbal and rule-governed behavior.

## Final Thoughts

One of the main arguments in favor of the metacontingency as a model for explaining social phenomena is that it embraces another "kind" of selection (cultural selection) beyond natural and operant selection which, despite being "emergent" on operant processes, would not be "reducible" to operant selection. As a consequence, cultural selection would demand a conceptual framework of its own, hence the metacontingency. Two basic premises can be located in those assertions, one ontological and one epistemological. The first is that we are dealing with a new kind of phenomenon – a new kind of selection process associated with the emergence and evolution of cultural practices. The second, which follows from the first, is that behavioral concepts and principles (as the three-term contingency) do not explain the maintenance and selection of cultural practices. Therefore, a new conceptual framework would be necessary. Our primary goal in this paper has been to argue that the epistemological premise present in the metacontingency literature is wrong. To do so, we presented pragmatic reductionism as a model to discuss the possibility of reductive explanations of selection and maintenance of cultural practices from metacontingency to contingency analysis. Based on this framework we provided examples of pragmatic reductive interpretations, thought experiments, and an analysis of experimental data in which we have tried to explain away the metacontingency.

We pointed out methodological problems regarding five examples of experimental research done within the framework of the metacontingency, but, most importantly, we also tried to reinterpret experimental data produced by metacontingency experiments using only contingency analyses. This can be tricky, however, because we did not have access to all the data necessary to do a proper in-depth analysis except for what was available in the published studies. The movement of open science (Hesse, 2018) and data sharing (Martone et al., 2018) is only starting in psychology. In the case of behavior analysis, we think there is no better field to start these movements than in the metacontingency experimental studies. Providing the raw data from these experiments would make the kind of analysis we attempted here more meaningful, no matter the outcome. Gilroy and Kaplan (2019) recently published a tutorial on how behavior analysts can use a particular service (GitHub) to make their raw data available. We think this is a promising path to take in order to discuss more comprehensively what is being investigated in the domain of "culturo-behavior science."

Still, the effort to try to explain the data without assuming new processes and/or adopting new concepts – that, for us, are clearly unnecessary – was and still is absent in metacontingency literature. The whole program started as a theoretical endeavor, with no empirical data supporting it (Zilio, 2019b); and when the experimental data finally came out, it does not seem to need the metacontingency to explain it at all. The metacontingency model as used (at least in the domain of interpretation, thought experiments, and the data of the experiments discussed here) seems to be easily explained away by considering well-established behavioral concepts such as rule-governed behavior, stimulus control, cooperation, choice, concurrent schedules, and the matching law. As Skinner (1953/1965) wrote: "if we are able to account for the behavior of people in groups without using any new term or presupposing any new process or principle, we shall have revealed a promising simplicity in the data" (p. 298). That seems the case here. Skinner (1953/1965) continues, however, cautioning that "this does not mean that the social sciences will then inevitably state their generalizations in terms of individual behavior, since another level of description may also be valid and may well be more convenient" (p. 298). Does he mean something like the metacontingency? We do not think so.

We argued here that the metacontingency epistemological premise is wrong, and that it is possible to explain the phenomenon that the metacontingency literature is trying to explain through contingency analysis alone. In sum, we think is possible to pragmatically reduce metacontingency explanations to contingency explanations. That does not invalidate, however, the ontological premise about the existence of different processes related to cultural evolution and selection, whatever those might be. It only shows that, if they exist at all, they are not the ones being studied in metacontingency research. This puts the metacontingency in an odd position, in a sort of conceptual "purgatory" one might say: the phenomena that are trying to be explained with metacontingency can be explained without it, so the studies are not exploring a new process in need of a particular conceptual framework. On the other hand, there might be social or cultural phenomena that indeed require a proper conceptual framework that goes beyond contingency analysis; however, that does not seem to be what metacontingency researchers are studying either.

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## Declarations

**Conflicts of interest** Authors Diego Zilio, Kester Carrara and Felipe Lustosa Leite declare that they have no conflict of interest.

**Ethical approval** This article does not contain any studies with human participants or animals performed by the authors.

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