When I was asked to discuss issues concerning agency and archaeology, my first reaction was that I had no very specific views; only a general notion that the agency concept was a Good Thing. I felt that in trying to respond I would be like an electron satisfied with a comfortably fuzzy and spread-out wave function, maybe here or maybe there, or almost anywhere, hit by an experimental probe and forced to collapse its wave function and decide matters hitherto left vague. But I soon realized that in fact I already had some quite sharp and strong opinions. I present some of them here, with special emphasis on the thorny issue of “rationality,” and suggestions for clarification of the sometimes vague concept of “context.”

Agency theory seems less a tool than a paradigm, almost a worldview. The Giddensian concepts of structuration, duality of patterning, and of individuals whose actions are in relation to circumstances (but not mechanically determined by circumstances) and which in turn have effects on circumstances (though usually not very large effects) seem to me the only way of thinking about present or past social phenomena that makes any sense.

If that were all there was to it, an “agency” outlook would be useful because it gives us some clear ideas of things to avoid. One is the notion that there are any human societies in which people normally follow custom unthinkingly, without often confronting situations that call for conscious choice. Another (beloved of old-line processualists) is that the adaptive response of societies to external stress is the key explanatory concept. A third is that individuals are not merely agents, but free agents, who can, at least in the social sphere, make just about anything they choose happen in their lives (a view especially vulnerable to feminist criticism; see Gero, this volume). Problems with all these ideas have been extensively discussed and I see no need to say much about them here. A view that does require comment is that something much like biological natural selection is the key explanatory concept.

Limits of Darwinian approaches

Approaches labeled “Darwinian” are very diverse (e.g. Barton and Clark 1997; Maschner 1996), and some may be compatible with agency approaches (see Clark, this volume). Under some circumstances, especially when a fairly large number of small politically autonomous societies coexist in a region, there can be a “cultural selection” process, in which some social/cultural/technological types of society become increasingly prevalent while others become less prevalent, in spite of strong intentions on the part of their members to socially reproduce their type. What Boone and Smith (1998) call evolutionary ecology does not seem wrong, but I think it is needlessly limited because it pays too little attention to, “undertheorizes” if you will forgive the term, individual agents.
However, I am highly critical of the "strict selectionist" version, associated especially with Robert Dunell and his students (e.g., chapters in Tellser 1995). Key concepts of strict selectionism, taken from biological evolution where they have been very productive, include variation and selection operating on variation. A crucial aspect of strict selectionism is that individual intentions have no explanatory value, at least not on the scales appropriate for archaeology. This is diametrically opposed to my view. On short-term time scales of a century or two, the intentions of individuals are important because they are an important source of variation, even if they are not the sole source. The source of biological variation is genetic mutation, and mutations are independent of selection pressures. This is emphatically not the case with human intentions as a source of sociocultural variation, because these intentions are significantly related to the contexts and perceived interests of individuals. The mind-set of strict selectionism diverts attention from contexts, resources, and interests, but that is exactly where attention should be focused, at least for short- and medium-term phenomena. Conflicting interests, conditioned by the different contexts of different individuals, are a major factor in intentions to reproduce or to alter existing sociocultural contexts. Many might go further and frame the matter in terms of dialectical tensions that drive change (e.g., McGuire 1992).

It could still be argued that, in the longer run, over millennia, it doesn't matter whether the sources of variation are random or structured by sociocultural contexts; selection will operate to favor some sociocultural types more than others. But social reproduction is an active process, and this means that perceptions of interests and the intentions that flow from these perceptions are constantly recreated. The fact that a particular type of situation may tend to generate some intentions more than others provides, in itself, in Darwinian terms, a form of selection, because it affects the relative frequencies with which different sociocultural phenomena are reproduced. These recurrently produced intentions are not necessarily the only source of selection, but there is no reason why they should be overridden by other sources. Even in the long term, recurrent intentions, conditioned by recurrent diverse situational interests, continue to count.

**Focusing agency**

Even if we avoid all these errors (action is overwhelmingly constrained by custom, action is virtually unconstrained, societal adaptation is the key, or selection is the key), we are still left with very vague ideas about what we should do instead. There are many varieties of agency theory, and some are better than others. To me, one key concept is that most people most of the time behave "sensibly" in regard to their perceived interests, in the circumstances in which they find themselves. Things do not happen just because someone wants them to happen, yet individual human intentions are at the root of both persistence and change in sociocultural forms. Furthermore, although sociocultural phenomena are not simply the sums of individual intentions, processes operating at supra-individual levels do not obliterate the effects of individual intentions and do not render intentions irrelevant to explanations.

In some discussions of agency, it is considered a property that only some individuals possess, or that is possessed to greatly varying degrees. I find this terminology unhelpful, and I prefer to think more about agents than agency. Every human is an agent. The same applies to the term "social actor," which sometimes seems to be used as if only a few people get to be actors and, as it were, the rest of us are stagehands or maybe just audience. I would rather speak of each of us as a social actor. None of this is to say that we are all equal. On the
Contrary, individual differences in resources, power, prestige, authority, and what might be called "social leverage" are critical, as are differences in personality and other psychological traits. This is so even in very small-scale societies. Resources, power, prestige, authority, dominance, resistance, and social leverage already seem perfectly good words for aspects of inequality. I think that to speak of agency in this sense is redundant at best, and may actually confuse issues.

**Insights from demographic anthropology**

One especially problematic variety of agency theory that is probably the real target of many critiques tends to equate agency theory with individualism and economic rationality approaches. These found particularly clear formulations in the works of Adam Smith and other Western economists and philosophers of the late 1700s to mid-1800s, and they survive (indeed, flourish) in the thought of many contemporary economists, although there are refreshing exceptions, such as Robert Frank (1988). In the economic literature I see a whole other conceptual world, and I often feel myself on the other side of a mental gulf as profound as that which separates me from creation scientists. Our bedrock assumptions, even our ideas of what counts as real, often seem too different to allow any fruitful synergy.

This attitude toward economic models is grounded in my personal experience. After an early start in physics, I shifted to anthropology in the 1950s, concentrating in archaeology. I did not do this because I found physics objectionable (as long as I wasn’t directly involved in developing weapons), but because I enjoyed archaeology more and felt I was better at it. I did have a vague feeling that applications of physical science discoveries were creating or exacerbating sociocultural problems, and while there seemed to be no shortage of physical science research, there did seem to be a shortage of social science research that might help us to deal with the sociocultural problems. By the 1970s I was worried about environmental and social problems connected with the immense increase in human numbers, and this led to an interest in demography. This had the not unwelcome side-effect that I learned some palaeodemography, but the main motivation had little to do with archaeology (which I did not then see as closely related to present day issues of ethics and right action), and I saw demography mainly as something very different from archaeology — like gaining skills in topics as disparate as Rumanian and mining engineering (Leacock 1930) — that I could find time to do and that would ease my conscience.

In fact, what I learned had unexpected payoffs for my thought about archaeological issues. I found out more than I liked about how wrong-headed economic demographers could be, and how effective their institutions and practices are at perpetuating this wrong-headedness. There is a strong tendency drastically to under-imagine differences in contexts. Lots of numbers about lots of people are accepted without paying enough attention to their validity, while qualitative data on a few people, no matter how valid, are dismissed as “anecdotal” and of little value. Johansson (1993), Hammel (1990), and Greenhalgh (1990) provide excellent discussions of these issues. Mamdani (1972), though a little overdrawn in places, is an early and entertaining exposé of the cluelessness of well-intentioned but anthropologically naive interventions. Sociologically-trained demographers tend to be better, but even they (e.g. Caldwell et al. 1987) emphasize ethnographic methods for collecting better data, and make less use of anthropological theory (cf. Hammel 1990).

I also found wonderfully instructive anthropological accounts of real people negotiating, planning, weighing alternatives, honoring or not honoring obligations, striving to maintain or improve their social standings, trying to reduce risks, seeking satisfactions, and so on, in
the situations in which they found themselves, and by means of the resources at their disposal. These studies concern behavior that is relevant for fertility rates, though not usually behavior calculated to achieve some target family size. Greenhalgh (1995) and Kertzer and Fricke (1997) are important recent edited volumes on anthropological demography. As a single outstanding example, Caroline Bledsoe and Fatoumatta Banja (1997) discuss how rural women in The Gambia (West Africa) perceive pregnancies as impairing their well-being and reducing their capacity for future child-bearing, whether they result in miscarriage, still birth, or live birth. This is especially so when a woman perceives her current physical condition as inopportune for pregnancy. What might look to an uninformed observer like a “natural pattern” of unregulated fertility is in fact the outcome of very conscious planning and reasoned efforts to conserve health and child-bearing capacity, to space and time pregnancies so as to maximize the number of living children. Thus, the issues are very different from those taken for granted in much of traditional demographic theory, such as whether another child can be afforded and the balance of drains and inputs children make on or to household finances. These other issues are also important to women in The Gambia, but the relevant context will be fundamentally misunderstood if one lacks knowledge of the sort that Bledsoe and Banja provide.

Refining rationality

Jenkins (1992) sensibly argues that Bourdieu is excessively critical of Rational Action theorists such as Jon Elster. Nevertheless, we must define “rationality” less narrowly, freed of the baggage that accompanies “rationality” in the senses used by economists and game theorists. In this broader sense, most people behave rationally most of the time, but we all have the experience of reflecting on past behavior and deciding that we have acted irrationally. Fatigue can impair judgement; strong emotions can override reason, often in rather patterned ways (overcome by rage, hunger, or sexual desire, we behave in ways we consider foolish and/or morally wrong; overcome by panic, we may flee a situation it would be more rational to face). It is interesting that, in English, the very term “overcome” implies that there is some normal, proper, rational way of behaving that is there to be overcome. I wonder how similar other languages are in this regard. In other cases, we can be inspired, by the examples and often the rhetoric of others, to be braver, more generous, or more aggressive than we would otherwise consider rational. A great deal more is going on in our minds than just “being rational” about things. Attempts to explain or understand sociocultural phenomena are hopelessly flawed if they do not take into account individuals and our propensity to make what we think are rational choices most of the time, but they will also be seriously incomplete if we do not give other mental phenomena their due. This is one of the reasons why developments in psychology are of such great potential importance for archaeologists (and all other students of social phenomena).

Palaeopsychology

Palaeopsychology never should have been derided. If agency is important we need to learn much more about agents, and this means learning much more about how human brains and minds work. This is a tremendously active field, in neuroscience, cognitive psychology, and evolutionary psychology, with notable contributions by archaeologists (e.g., Mithen 1996). There are many strongly-held views, often at odds with one another. However, I am sure that archaeologists will be missing something of major importance if we do not keep up
with developments in this field. I am also sure that many inbuilt capacities and propensities will be found; old models of the inexperienced human mind as a blank slate that passively absorbs inputs (e.g., Locke) are hopelessly inadequate. This does not mean that mental activities are rigidly specified, but it does mean that some kinds of mental things are much easier to learn and easier to do than others (and harder to unlearn or avoid doing). It is also highly likely that the human brain – the organ that produces the activities we call “mind” (mislabelling a set of processes as a thing) – is fairly modular, yet has a high capacity to link across modules. A tremendous amount remains to be learned about the number and nature of these modules. This new knowledge will enrich our ideas about ancient agents.

**Reasoning**

We must be extremely careful about what aspects of rationality are universally applicable, and which aspects are more context-specific. It is hard to avoid a definition that does not boil down to “thinking about it the way I would,” or “getting the ‘right’ answer.” There is a fairly distinct kind of mental activity (though with the usual fuzzy borders) that can be called “reasoning,” and it means things like weighing alternatives, and trying to figure out the likely consequences of doing this or doing that. It contrasts with other kinds of mental activity, such as reflex reactions, feeling physical sensations, feeling emotions, arousing emotions, noticing things and not noticing things, any of which may go on concurrently with reasoning. What I mean by “rationality” is “using reasoning as a major factor influencing action.” The reasoning may involve assumptions that I (and probably you) consider faulty, and/or what I (and probably you) consider mistakes in logic. The hallmark of reasoning is not the “correctness” of the method or the result, but the type of mental activity involved.

I believe reasoning-type mental activity is a human universal and, while I think I see signs of rather simple reasoning in my cats, I think highly developed reasoning abilities are a distinctively human mental capacity. Furthermore, although I have been careful not to define reasoning and rationality in terms of getting the correct answer, I believe that reasoning, for most people most of the time, in situations that are not too unfamiliar to them, leads them to a pretty good answer. That is, it is helpful to them in making choices that improve their chances of attaining something close to their conscious goals and furthering their perceived interests. I am not a sociobiologist, yet I don’t think that we would have evolved elaborate reasoning abilities if they did not tend to increase biological fitness for our ancestors. This implies that there are some common denominators to human reasoning. There is a rough analogy with speaking; speech is a human universal, while what we talk about and the languages we use vary widely. So with reasoning; we all reason, but what we reason about and the procedures we use may differ greatly. The allegedly universal rationality assumed by “economic man” models is shown by anthropological knowledge to be the very opposite. Its image of highly individualistic actors whose only important mental processes are reasoning in relation to explicit goals, is, in fact, exceptionally culture-specific and ethnocentric. It is probably also highly androcentric, which is one basis for claims that agency approaches are androcentric (see Gero, this volume).

The problem with so-called universal rationality lies less in the emphasis on reasoning than in the belief that it is decontextualized when in fact it is exceedingly context-specific; specific to certain western (and perhaps male) contexts. The women discussed by Bledsoe and Banja are probably not being less (or more) rational than capitalist CEOs; they are
being rational in regard to different perceived issues, in relation to different contexts, different experiences, and different knowledge.

Perceived interests

"Perceived interest" is another key concept. I stress "perceived" to emphasize that, although there is a universal core of interests connected with sheer survival, further interests are largely socially constructed, and even those interests closest to sheer animal well-being are conceived in ways that can be strongly structured culturally. Equally important is that no two individuals will have quite the same perceived interests, although members of particular class, gender, and factional categories may have many in common (as emphasized by Brumfiel 1992). To the extent that individuals in such a category engage in coordinated action based on perceived common interests, it is probably sometimes useful to think of the whole category as a sort of "super-agent." In such cases, research might focus on how coordinated action is brought about and maintained, and the extent to which the interest group can hold together and avoid defectors—(cf. Hechter 1987 on differences between party discipline in British and US politics).

Contexts

Contexts have physical and sociocultural aspects. By "physical" I mean the natural environment and the technological means for interacting with it—the sorts of topic archaeology has focused on especially and which remain important—not superseded by a focus on individual actors. There has been some tendency to overemphasize socially constructed aspects of contexts, as if what you don't see or don't believe can never be important. I am a philosophical realist. Very different attitudes toward and concepts of phenomena such as time, life, death, procreation, landscapes, food, and so on must be recognized, yet we cannot disregard aspects of these phenomena that are real, whether or not any minds are there to perceive them.

By "sociocultural" aspects of context I mean all the social institutions and ideas in which an individual finds himself or herself situated. This includes Bourdieu's "habitus" and, I think, "structures" in Giddens' sense. They are historically contingent but fairly widely shared among an age, gender, and status category in some particular time and place. One's actions are not simply dictated by these institutions and ideas, but one unavoidably acts with regard to them, obeying, questioning, resisting, manipulating, or whatever.

A third aspect of context is the specific individuals and material circumstances with which one interacts. For example, concepts of what is right and proper and also what is likely or unlikely to occur are part of the sociocultural aspect of context, as are notions about relations between older and younger sisters. The fact that I as an individual have two older sisters and one younger, each with such-and-such characteristics, is part of this third aspect of context, which might be called one's situation.

Another way of sharpening thought about context is to distinguish between external and internal. External contexts exist outside persons, but an important part of one's internal context is one's perceptions and feelings about the external context.

Relations between individuals and their contexts are often considered difficult to theorize. I am a little uneasy about this. In the abstract, it seems dead simple: individuals' perceived interests are strongly affected by their contexts, yet individuals must still make
choices, and the outcomes of action based on these choices, intended or not, will affect the context. People are affected by their contexts and people have effects on their contexts.

Explaining change

Moving down from this grand abstract level to understand or explain specific cases of sociocultural change (or the lack of it) seems, at present, more an ad hoc matter of trying to reconstruct what happened in specific cases and suggesting reasons that seem to make sense in terms of individual/context interactions. Further theorizing about the relationship seems unproductive except in the light of such case studies.

Lack of change needs explaining as much as does change. If received cultural rules are never so strong as to prohibit choosing and maneuvering in the light of one's perceived interests, then lack of change cannot be explained as "cultural inertia." There is always active cultural reproduction. Sociocultural changes, especially rapid ones, often happen when individuals or a group with enough influence intend to make change, even though the results are often not, or not just, those intended. Change also happens for other reasons, including slow change that is merely the result of imperfect replication of received traditions. Also, some change really is due to outside factors; we should not over-react against attempts to explain all change as exogenous.

Absence of change also has to be produced, and it implies that incentives for keeping things the same, or disincentives for change, are repeatedly created and re-created over many generations. Since individuals in different situations have different incentives, lack of change implies that, for a long time, the efforts of those who dissent from the status quo, or actively work against it, may cause disturbances but lead to no structural changes; resistance for sure, rebellions perhaps, but not revolutions.

It is undesirable to label a lack of change "stability" or "stagnation." The former smacks of systems models and has a baggage of positive value appraisal. The latter implies a "progress" model and has pejorative overtones. Both divert attention from the point that sociocultural reproduction is always an active process, whether or not the received form is reproduced almost exactly or with considerable change.

How can archaeologists apply the concept of rationality?

So, how does any of this actually apply to archaeology, especially to prehistory? Nothing I have said about individuals reasoning sensibly in regard to their perceived interests and contexts depends on the use of written records (though writing provides new techniques for reasoning and enlarges the scope for rationality). Therefore, it is just as applicable to the past (at least the last 30,000 years) as to the present (e.g. see Wobst, Sinclair, this volume). Alternative paradigms that will not work for the present cannot work for the past either. However, prehistory gives us fleeting and disconnected glimpses of individuals and individual actions; only in a very limited way can we get at individual biographies (as with skeletal and dental markers that reflect past episodes of vaguely-defined stress), and this seems to make reasoning individuals largely invisible archaeologically. I have no problem, however, with postulating entities that we know are there, even if we cannot detect them. I reject the contrary ontology that says that if we cannot detect something it is not there, or at least we must not think about it. This belongs to the sort of extreme logical empiricism that is now rightly discarded.

Rationality is somewhat context-specific, but not totally so. Rather than leaving it vague
and muddy, can we steer a nuanced and well-reasoned course between these extremes? I offer a few suggestions.

There seem to be recurrent types of context, and within a particular type of context there are recurrent situations with regard to which people often reason. There are recurrent concerns, tensions, even contradictions. Relevant dimensions include the type of household/family (nuclear, extended, or something else), the difficulty or ease of dissolving marital unions, the relative status of men and women, considerations involved in forming new household units, modes of subsistence, and relations between elites and commoners. One type of context might be agrarian households composed of extended families where marriage is difficult to dissolve, women are subordinated to men and juniors to seniors, good land is in short supply, and landlords or the state extract heavy rents. Another type could be foraging households consisting primarily of a married couple and their children, with little gender-based difference in status, easy divorce, no rents or taxes, and considerable sharing among households. I emphatically do not set these up as tight packages of traits that always go together; they are simply examples, and there are all sorts of variations and different combinations of these and many other variables. My point is that at least some of the variables I have listed are archaeologically accessible. A particular configuration of these variables does not determine the issues about which people will recurrently reason, nor just how they will perceive their interests, but it does seem to narrow the range of what is likely. It suggests things archaeologists should look for and think about.

Suppose archaeologists had excavated a reasonable sample of the remains of a community of the society discussed by Bledsoe and Banja (1997), but had no other knowledge of it. Would they entertain the possibility that women might have been concerned with the issues that Bledsoe and Banja report and that women were practicing similar reproductive strategies? I hope so. Certainly anyone who reads this article should. And simply being aware that this might have been the case is important, because it broadens the range of possibilities to be taken seriously, and helps to avoid uncritically projecting modern Western assumptions into the past. Would archaeologists be able to go further, and gain evidence that women probably were pursuing such strategies? At present this is a sticky point; I'm not sure that we could, but once the issue is clearly framed, if we think about the matter enough perhaps we could see how to gain such evidence.

We can also ask whether it matters whether the ancient people were concerned with these issues and pursued similar practices. It does matter. It matters, for example, to any proposed explanation of why fertility and population were increasing, decreasing, or staying about the same, and causes of population change are frequently discussed issues in archaeology.

How do I use agency theory in practice?

In the space available I will not present any extended case studies; examples pertaining to the ancient city of Teotihuacan can be found elsewhere (Cowgill 1979, 1983, 1992a, 1992b, 1997). However, I will comment briefly. “Agency theory” does not give me a recipe or algorithm for interpreting finds, and I use many resources other than agency theory. These include ethnohistoric materials and the belief that ancient meanings of symbols are often somewhat related to their more recent meanings in Mesoamerica, even if meanings are prone to modification when contexts change and when the symbols serve different interests. Nevertheless, agency theory “informs” all my thought about the past. Partly this is because it makes me highly skeptical of proposed explanations that invoke any of the assumptions I have criticized earlier (that action is highly constrained by custom, action is
unconstrained, adaptation is key to explanation, selection is key, decontextualized rationality is key). For example, some assume that only the prospect of upward social mobility through prowess in war could have motivated ordinary Teotihuacanos to serve in armies. This is a narrow view of rationality that underestimates the extent to which moral sentiments, religious beliefs, “esprit de corps,” and other factors can motivate young men to be soldiers.

Most of all, an agency outlook affects the questions I think it most interesting to ask, and also my ideas about what is intrinsically likely, even though I hope I avoid the fallacy of assuming the truth of propositions that I’m ostensibly testing. Thus, for Teotihuacan, key questions include the means by which so many people were brought together to build such monumental civic-ceremonial complexes and to live in a rather ordered fashion. Instead of assuming universally shared norms, I am led to wonder what sorts of political negotiations and struggle went on behind what seem to be public proclamations of harmony, what led up to the eventual violent iconoclasm that marked the end of the Teotihuacan state, and how, for centuries before that time, day-to-day relations of production and distribution managed to work reasonably well. I do not have the space even to sketch tentative and complex answers here. The point is that these different questions have an effect on research priorities and strategies; on the kinds of evidence one looks for. This is the greatest merit of an agency outlook, not just that we look at old evidence and old kinds of evidence in new ways, but that our eyes are opened to new things to look for.

Acknowledgments
This chapter has benefited from comments on an earlier draft by Marcia-Anne Dobres and John E. Robb, Michelle Hegmon (who, among other things, called my attention to Jenkins’ excellent book on Bourdieu), and John Kunkel. I take, of course, responsibility for the final version.

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