



Measuring change

As Cadenasso *et al.* (*Front Ecol Environ* 2007; **5**[2]: 80–88) remind us, measuring fine-scale landscape structure remains a critical challenge in investigating the ecology of coupled human–natural systems. Human interactions with ecosystems take place within highly heterogeneous and dynamic fine-scale landscape mosaics, especially in urban and other densely populated anthropogenic landscapes. Tools that can measure ecological pattern, process and change at fine spatial scales and link these with human activity are therefore essential for investigating how coupled human–natural systems function and change (Rindfuss *et al.* 2004).

Given this broader goal, it is hard to see how the authors' proposed land cover classification can advance ecological understanding of anthropogenic ecosystems. The system's land units, "HERCULES patches", are not fine-scale landscape features; they are highly abstracted spatial units that lump together most of the landscape features recognizable to land managers and ecologists in the field, such as trees, yards, buildings, and roads (Figure 1). As a result, these units are no more useful in measuring land management, ecological processes, or changes in these than the conventional coarse-resolution land units (30-m pixels) of most regional land classification systems. Local land managers cannot recognize these units, so they do not aid in collecting land management data or in linking

these data to distinct parts of the landscape. The same is true for ecologists: the spatial scale of these units is simply too large to support most field-based observational efforts directly.

What precisely do "HERCULES patches" represent from an ecological perspective? The only application presented by the authors – correlating their manually-classified land cover units against an ecological variable (stream nitrogen) – seems a fairly minor application, especially when continuous data for imperviousness and tree canopy cover are readily available from remote sensing (Vogelmann *et al.* 2001). Mixing land use and land cover does indeed cause problems (eg erroneous cropland identification in Figure 1), yet Cadenasso *et al.* propose that we ignore land use altogether. While this may offer technical advantages, it overlooks the key functional importance of land use in anthropogenic landscapes: land cover is a structural measure only. Investigating relationships among land cover, land use, and ecological processes is important, but this does not mean that land cover units are a sufficient substitute for units based on ecological function.

We agree with the authors that understanding the ecology of entire cities is necessary to make progress in urban ecology. But cities are not discrete objects. They are networks of human settlements embedded at multiple scales within regional mosaics of agricultural, forested, and other managed lands (Ellis and Ramankutty 2008). Can the structure and dynamics of cities be understood without relation to this? By treating urban ecosystems as entities unto

themselves, differentiated only by land cover, Cadenasso *et al.*'s review, and HERCULES in particular, ignore the structural complexity of cities and their integration within regional landscapes, while requiring observers to ignore basic differences in land use, viewing for example, all grassy areas as ecologically identical, whether they be yards, wheat fields, pastures, or golf courses.

No single tool can be optimal for all investigations. Yet the approach advocated by Cadenasso *et al.* seems to offer more limitations than advantages. Not the least of these is the lack of any system for quantifying errors in its products, a prerequisite for change measurements and cross-site comparisons (Ellis *et al.* 2006). Statistically rigorous observing systems that can facilitate measurements of functional linkages between human and ecological processes at multiple spatial scales will help us to advance the science of coupled human–natural systems (Rindfuss *et al.* 2004; Liu *et al.* 2007). Though all efforts in this area are laudable, the land classification system advocated by Cadenasso *et al.* seems ill suited to advancing this goal.

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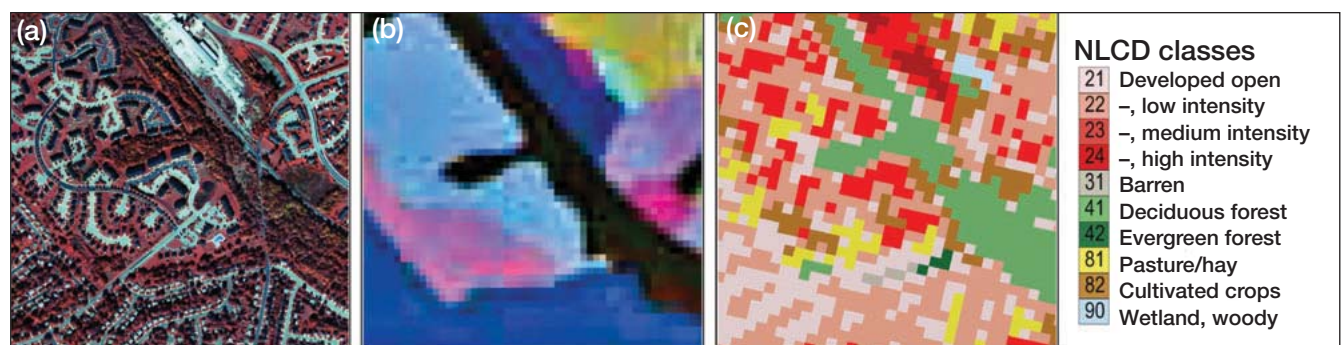


Figure 1. (a) False color aerial photograph, (b) "most finely resolved" HERCULES patches (Figure 7d from Cadenasso *et al.* 2007), and (c) NLCD pixels (Vogelmann *et al.* 2001) across a 1-km² sample of suburban landscapes near Baltimore, Maryland.

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The authors reply

We agree with Ellis that refinement of land-cover classifications of urban systems is needed and that no one tool is optimal. We disagree, however, with his other conclusions and feel that he has misread our work.

HERCULES aggregates fine-scale elements and differentiates patches using the types and cover of those elements. Classifications based on 30-m resolution imagery (eg National Land Cover Database [NLCD]), cannot account for fine-scale elements or allow “mixed” pixels (Figure 1). Comparing the aerial photo (Figure 1a) and NLCD (Figure 1c) classification shows that NLCD incorrectly describes residential lands as cultivated crops and pasture/hay. Integrating built and natural components in HERCULES is intended to more accurately represent coupled human–natural systems. Ellis’ assertion that HERCULES patches are not recognizable by land managers is unsupported. Managers have not yet applied or evaluated

HERCULES. However, multiple observers recognize the same land cover shifts in air photos, HERCULES maps, and field reconnaissance.

Ellis is confused about what HERCULES patches represent. They combine the basic urban cover elements – vegetation, buildings, and surfaces (Ridd 1995). These elements affect ecological functions. An example in our paper acknowledged as preliminary, demonstrated that HERCULES better predicted watershed nitrate yield than the NLCD.

Contrary to Ellis’ criticism, we did not suggest that land use be ignored. Rather, we argued for the separation of land use and cover so that relationships between urban ecosystem structure and function can be tested. When use and cover are confounded, as in most classifications, rigorous structure–function analyses cannot be conducted. As stated, land use and other data can be added to HERCULES as required by the specific research question. This facilitates greater flexibility to match variables and scales between research questions and data than using “off-the-shelf” classifications. A challenge in urban systems is the mismatch resulting from the existence of parcel-based land use and the absence of equivalently scaled land-cover data. Hence, scalable models of land-cover are fundamental to our understanding of urban ecosystems.

We stated that “urban” refers to the range of habitats within extensive metropolitan regions. Because HERCULES includes vegetation, it is clear that we intend it to represent

the interdigitations of built with non-built areas in urban cores and fringes. Against Ellis’ claim that we are “ignoring the structural complexity of cities”, we emphasize the opposite.

It is incorrect to say that there is a “lack of any system for quantifying errors in its [HERCULES]’ products”. Space prevented us from fully exploring assessment, which can be done in two ways. First, the accuracy of the patch delineation can be quantified using discrepancies between patch delineations of trained interpreters. Second, the accuracy of the proportion for each of the five landscape elements can be measured by comparing proportions assigned by visual interpretation to automated object-oriented classifications, which is currently underway. HERCULES is well suited for detecting change in landscape structure because changes in either vegetation, buildings, or surfaces – not just the change of coarse categories, such as residential to another land-cover type – can be detected. HERCULES provides spatially exhaustive sampling of urban areas rather than sampling its subsets.

Our framework, based on scale and degree of integration, suggests that HERCULES is not the only or the best tool for describing the structure of urban systems, but that it is one option to fill a void left by the available classifications. Each tool will work best for different research questions, and it is important to select tools appropriate to a particular question rather than using what is readily available without evaluat-

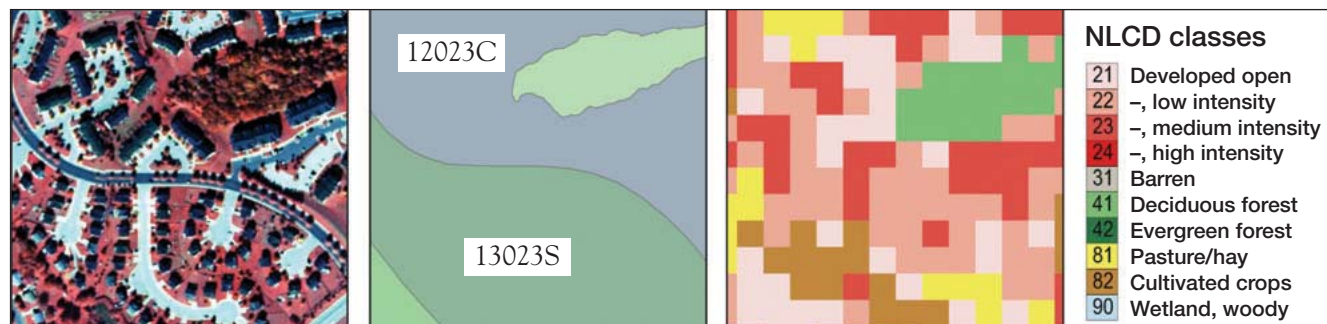


Figure 1. Region depicted in Ellis’ Figure 1. (a) False color infrared image of submeter resolution and (b) HERCULES classification of the image. The two patches differ from each other in building type (single [S] versus connected [C]) and in the proportion cover of fine vegetation, which is higher in the patch with single structures. See Cadenasso *et al.* (2007) for full description of patch nomenclature. (c) The NLCD classification of the same area.

ing the underlying assumptions and constraints.

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Ridd MK. 1995. Exploring a V-I-S (vegetation-impervious surface-soil) model for urban ecosystem analysis through remote sensing: comparative anatomy for cities. *Int J Remote Sens* **16**: 2165–85.



Compensatory mitigation

My previous letter (*Front Ecol Environ* 2007; **5**[8]: 407–08) highlighted some problems associated with the notion, posed by Wilcox and Donlan, of using compensatory mitigation to offset the impacts of seabird bycatch in fisheries (*Front Ecol Environ* 2007; **5**[6]: 325–31). Wilcox and Donlan used, as the case study to justify their model, the population of flesh-footed shearwaters on Lord Howe Island (LHI). The model assumes that removal of rats from LHI will substantially enhance the shearwater's breeding success. I pointed out previously that there is no evidence that rats are suppressing the reproductive productivity of this species. Indeed, the available evidence, albeit equivocal, suggests otherwise. The premise on which the model is based is therefore not only unsubstantiated, but is probably false; eradicating rats from LHI will not offset flesh-footed shearwater mortality in longline fisheries.

In their reply to my comments (*Front Ecol Environ* 2007; **5**[10]: 521–22), Wilcox and Donlan reiterated their belief that the reproductive productivity of flesh-footed shearwaters is being reduced by rats. They claim that “there is ample evidence that rats may be impacting flesh-footed shearwaters on LHI” and “based on available evidence, it is reasonable

to expect large increases in breeding success with the eradication of rats”. I contest both of these assertions.

There is no doubt that rats have caused the decline or extirpation of many seabirds on many islands, and the removal of these rats has led to the recovery of some seabirds, as well as other plants and animals. However, seabirds show variable responses to rats. Some populations have been extirpated, others have remained relatively unaffected. The level of impact can vary according to rat density and species, the size of the island, habitat, food availability, and the presence of other exotic species.

The Norway rat (*Rattus norvegicus*) generally has a far greater impact on ground-nesting seabirds than does the smaller black rat (*Rattus rattus*). Black rats, being far more arboreal, tend to cause greater harm to terrestrial birds. In general, the vulnerability of seabirds is inversely related to body size; smaller species are more vulnerable than are larger species, although there are exceptions. In a recent review, Towns *et al.* (2006) concluded that Norway rats reduced recruitment in burrowing seabirds of up to 750 g in body weight, whereas black rats generally affected burrowing seabirds weighing less than 260 g.

Wilcox and Donlan cite increases in populations of little shearwater (*Puffinus assimilis*) and Audubon's shearwater (*Puffinus lherminieri*) after the removal of rats to support their case, but these species are much smaller than the flesh-footed shearwater, and are within the range susceptible to predation by black rats. Flesh-footed shearwaters (580–750 g) are too large to be highly vulnerable to predation by black rats. Evidence from studies of flesh-footed shearwaters on both LHI, New South Wales (Priddel *et al.* 2006), and Woody Island, Western Australia (Powell 2007) support this conclusion. Both studies show reasonable levels of breeding success ($\geq 40\%$) and no evidence of predation by rats. Wilcox and Donlan argue that the lack of evidence of rat predation on flesh-footed shearwaters is equivocal. Maybe, but there is no

direct evidence to suggest that predation of eggs or chicks is occurring, let alone at levels necessary for compensatory mitigation to offset the mortality of adults killed by longline fishing. On LHI, the providence petrel (*Pterodroma solandri*), a bird whose size is similar to that of the flesh-footed shearwater, is increasing its distribution and abundance, despite the presence of rats. Even small species, such as the little shearwater, black-wing petrel (*Pterodroma nigripennis*), and white tern (*Gygis alba*), are all increasing in number (Hutton 1991; Hutton and Priddel 2002; Priddel *et al.* 2003).

Not only is compensatory mitigation for marine bycatch inappropriate for flesh-footed shearwaters, but it probably has limited applicability for most seabird species killed in longline fisheries. Seabirds killed on longlines are generally large, whereas the seabirds most impacted by rats are small. Thus, compensatory mitigation is likely to be appropriate for only a few (if any) seabird species. The fact that the flesh-footed shearwater was chosen as the case study, rather than a species in which the interactions and benefits are clear cut, reflects the dearth of suitable candidates.

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