# LETTERS

*edited by Jennifer Sills*

## The Legitimacy of Genetic Ancestry Tests

I FOUND THE POLICY FORUM "THE SCIENCE AND BUSINESS OF GENETIC ANCESTRY TESTING" (D. A. Bolnick *et al*., 19 October 2007, p. 399) to be a shallow discussion of a very complex topic.

Bolnick *et al*. criticize one company in particular—DNAPrint Genomics, Inc.—for promoting the idea that race is rooted in one's DNA (*1*). In fact, DNAPrint Genomics takes great pains to explain to customers that both genetics and race are imperfectly correlated with geography. DNAPrint's Web site (*1*) and print publications (*2*) clearly explain that the bases for human-derived notions of "race" incorporate genetics as well as geography, religion, culture, and even socioeconomics. Bolnick *et al*., however, deceptively imply that genetics does not constitute even a component of the equation.

Bolnick *et al*. also allege that genomic ancestry panels present a biased picture of non-neutral mutations, which is not the case (*2*, *3*). They then imply that genomic ancestry methods rely on imperfect—i.e., insufficiently large—databases and thus produce misleading results. However, the onus on the database developer is not to build a perfect database, but rather to quantify how imperfect the database is. DNAPrint Genomics has spent enormous resources doing just that (*2*).

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Likewise, Bolnick *et al*. focus on the fact that alleles are continuously distributed, and that "companies sometimes fail to mention that an allele could have been inherited from a population in which it is less common." This statement is misleading. The continuity of allele frequencies is the reason why, when estimating genomic ancestry with respect to a given population model, we use large numbers of well-characterized markers appropriate for that model, with algorithms capable of accommodating uncertainty. DNAPrint Genomics has gone to great lengths to determine confidence intervals and to quantify the bias and mean square error of our estimates. If a customer's likelihood of ancestry from a certain parental population is very small, the customer's report will reflect this.

I also object to the implication that the detection of lower levels of "Native American" ancestry in Asia and Europe is evidence that the AncestryByDNA test is illegitimate. Native Americans are derived from southwestern Siberia and central Asia, an area of the world that likely also contributed through population expansions and migrations to many other populations (*2*, *3*). Numerous "Native American" Y and mtDNA haplogroups have been found in Europe and Central Asia [figures 4-3 and 4-4 of (*2*) and (*4*, *5*)]. The confusion lies in the choice of the term "Native American." Naming parental populations with descriptors based on modern-day populations might lead to misinterpretations (2), but DNAPrint Genomics makes every effort to explain this complex topic to a lay customer base (*1*).

I am also offended by the implication that scientists who work for companies are corrupt. It is wrong to assume that any science coming from a company is suspect because money is the motivator of its generation, while overlooking the possibility that an academic scientist may falsify data in order to procure a grant, ensure tenure, or otherwise enhance stature or financial position. Generalizing about individuals on group membership in this way is the intellectual equivalent of bigotry.

Bolnick *et al*. believe that anyone who says they belong to a group should belong to that group—regardless of whether or not their deep ancestors (as reported by DNA tests) were part of the parental population associated with that group. The irony is that we do not disagree. In some cases, genetic testing is simply not relevant—not because it is flawed, but because it reports only one aspect of "race" or "ethnicity." Genomic ancestry tests demonstrate that admixture is the rule rather than the exception and hence support that idea that human-derived notions of "race" are based on the subjective and ever-changing concepts of social and political identity.

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### Response

OUR PURPOSE IN WRITING THIS POLICY Forum (19 October 2007, p. 399) was not to persuade the public that tests for genomic ancestry are wholly illegitimate, as Frudakis assumes. Rather, it was to call attention to this influential commercial enterprise and the need for consumers (and the public) to better understand the capabilities and limitations of the available tests. We also hoped to inspire genetics and anthropological associations to discuss these issues with their members.





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We did not suggest, as Frudakis states, that there is no connection between genetics and societal interpretations of race. Racial identity is shaped by a variety of factors, including social relationships, life experiences, and biological ancestry. Although DNAPrint's Web site states that race reflects more than genetics, it still (as of 2008) leads consumers to believe that race is inscribed in one's DNA. DNAPrint defines the "biogeographical ancestry" measured by their test as "the biological or genetic component of race" (*1*), and their underlying model reinforces the archaic racial view that four discrete "parental" populations existed in the past. The assertion that there is some sort of discrete genetic component to race is problematic, and there is no evidence that only four isolated populations existed at any point in the evolutionary history of our species (*2*, *3*). Furthermore, an extensive derivative literature makes it clear that many people think that the AncestryByDNA test identifies their racial makeup (*4*–*7*).

Frudakis asserts that our Policy Forum claimed that genomic ancestry panels are rife with biased, non-neutral mutations. We made no such claim. Instead, we noted that some ancestry informative markers (AIMs) involve loci that have undergone selection (*8*). On the basis of the information provided by DNAPrint Genomics, it is clear that some AIMs are skin pigmentation alleles and others are blood protein alleles involved in malarial resistance (*1*, *4*, *9*–*11*). It is therefore important to consider whether these markers measure ancestry alone, or whether they also reflect shared environmental exposures (and thus are not always indicative of shared ancestry). Because the AncestryByDNA test does not differentiate between different evolutionary reasons for shared alleles, the test results may be misleading.

Frudakis then objects to the suggestion that genetic ancestry tests are problematic because they may yield incomplete results due to limited sampling. He is right that perfect databases will never exist, and we agree that companies should quantify the uncertainty and limitations imposed by their databases. DNAPrint Genomics does calculate the bias, error, and confidence intervals of their estimates, but incomplete geographic sampling creates systematic bias that is difficult to quantify statistically. The sampling of a few, widely dispersed populations for marker selection likely influences the test's results. It is also U.S.-biased because it represents a specifically American racial understanding of human difference. Furthermore, most mitochondrial DNA and Y-chromosome tests do not provide any such statistics, so consumers are often unaware that those tests may yield incomplete or uncertain results.

Frudakis suggests that the problem with the AncestryByDNA test is not that it detects shared ancestry between Native Americans and Eurasians, but that this ancestry is referred to as "Native American." We agree. Because the shared alleles predate the divergence of these populations and likely originated in Central Asia, it is misleading to use them as markers of "Native American" ancestry.

Finally, it is clear that scientists who work for companies are not inherently more easily corruptible than academic scientists. That said, there are differences in how academic and commercial products are evaluated. Peer review may be imperfect, but it does require academics to convince experts that their conclusions are supported by the data. Commercial products are not subject to the same system of peer review, and it can be difficult to evaluate conclusions based on proprietary databases. Consequently, conflicts of interest may lead to different outcomes in academia and the business world.

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## Letters to the Editor

Letters (~300 words) discuss material published in *Science* in the previous 3 months or issues of general interest. They can be submitted through the Web (www.submit2science.org) or by regular mail (1200 New York Ave., NW, Washington, DC 20005, USA). Letters are not acknowledged upon receipt, nor are authors generally consulted before publication. Whether published in full or in part, letters are subject to editing for clarity and space.

# Soil Erosion: A Carbon Sink or Source?

THE REPORT BY K. VAN OOST *ET AL.*, "THE impact of agricultural soil erosion on the global carbon cycle" (26 October 2007, p. 626) raises two serious concerns.

First, the eroded soil is severely depleted of its soil organic matter (SOM) pool (*1*–*3*), which is preferentially removed by surface runoff because it has low density and is concentrated in the surface layer. Along with the SOM pool, severely eroded soil loses its nutrient and water retention capacity. Thus, an eroded soil generally has lower productivity even with additional input of chemical fertilizers and organic manures (*4*, *5*). Because of low productivity and reduced return of root biomass, the rate of replacement of SOM pool is low on severely eroded soils. This is in contrast to the assumptions made by these and other authors (*6*–*10*).

Second, the process of soil erosion by water entails three distinct stages: (i) detachment, (ii) transport/redistribution, and (iii) deposition. The first two stages lead to breakdown of structural aggregates and exposure of the hitherto encapsulated SOM within the aggregate to microbial processes with an attendant increase in mineralization and emission of $CO_2$ (*3*, *11*–*13*). During the third depositional stage, the rate of aerobic decomposition may be low and reduce the emission of $CO_2$. However, prevalence of anaerobic conditions accentuates methanogenesis and denitrification leading to efflux of $CH_4$ and $N_2O$ (*14*), with the relative global warming potential, respectively, of 21 and





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310 times that of $CO_2$. Taking all three stages into consideration, soil erosion is a strong source rather than sink of atmospheric $CO_2$, and it also exacerbates the problem of nonpoint source pollution and hypoxia in coastal zones. With severe adverse impacts on agronomic production and food security (*15*), especially in sub-Saharan Africa and South Asia (*16*), it is important that the accelerated soil erosion is effectively controlled and eroded soil judiciously restored.

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## Response

WE AGREE THAT ACCELERATED SOIL EROSION should not be promoted as an effective sink for atmospheric $CO_2$, and we want to emphasize the need for soil erosion control and restoration. However, our results do not support Lal and Pimentel's assertion that agricultural soil erosion is a strong source of atmospheric $CO_2$.

First, we demonstrated that the carbon content of eroding soils is subject to continual decline as carbon is exported from the site of erosion. It is true that selective erosion of the fine fraction may further accelerate this export. However, severe soil erosion will almost always take place as tillage, rill, and/or gully erosion, which is nonselective (*1*). This local decline does not imply that erosion is a source of atmospheric $CO_2$ because a fraction of the eroded C was replaced by enhanced stabilization of C in the newly exposed soil, resulting in a net uptake of atmospheric $CO_2$. Although it is true that erosion may reduce crop yields and hence inputs of carbon at sites of erosion, our mass-balance method accounts for all mechanisms occurring (including a yield decrease) and is not based on any assumptions about productivity or soil organic matter (SOM) replacement rate. Furthermore, the replacement rate of SOM was low on eroded sites, resulting in a small rather than a large sink, but a sink nonetheless.

Second, Lal and Pimentel correctly stated that the fate of SOM during the various phases of the erosion-deposition process should be considered, which is what we did in our analysis. However, the affirmation that detachment and transport of SOM inevitably lead to its accelerated decomposition is incorrect. In contrast, consistent with other studies, our results showed that very little net $CO_2$ release occurred after transport and burial within our microcatchments (*2*–*4*).

Finally, we agree that $CH_4$ and $N_2O$ from depositional sites warrant further research, but our report focused on the carbon cycle and therefore did not consider $N_2O$ fluxes. The majority of agricultural soils are mineral soils, which are a sink rather than a source for $CH_4$ (*5*). Although depositional sites in agricultural fields are wetter, it is unlikely that they will often be flooded and reach a low enough redox potential for methanogenesis (*6*).

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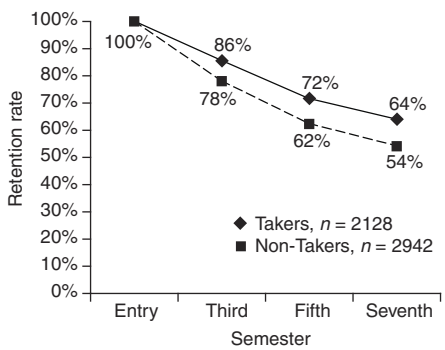
## CORRECTIONS AND CLARIFICATIONS

**News Focus:** "Scientists hope to adjust the president's vision for space" by A. Lawler (1 February, p. 564). Molly Macauley was expressing views offered by participants at last fall's National Research Council forum in Irvine, California, rather than her own opinion.

**Random Samples:** "When worlds collide" (7 December 2007, p. 1531). The item incorrectly stated that astronomers estimate 1 in 1000 stellar systems host planet-forming dust belts. In fact, that is the number of stars around which astronomers currently see such short-lived belts; many more stars must have them at some time in their history.

**Education Forum:** "Engineering education research aids instruction" by N. L. Fortenberry *et al.* (31 August 2007, p. 1175). The same data represented by the original figure are regraphed with the *y* axis extended to zero (right).




## TECHNICAL COMMENT ABSTRACTS

### COMMENT ON "International Conservation Policy Delivers Benefits for Birds in Europe"

**Rolando Rodríguez-Muñoz, Alfredo F. Ojanguren, Tom Tregenza**

Donald *et al.* (Reports, 10 August 2007, p. 810) assessed the impact of the European Union's Birds Directive, a conservation policy enacted in 1979, and reported evidence for positive population changes in targeted species. We argue that their conclusions are overstatements based on unsuitable data and inappropriate analyses.

Full text at www.sciencemag.org/cgi/content/full/319/5866/1042b

### RESPONSE TO COMMENT ON "International Conservation Policy Delivers Benefits for Birds in Europe"

**Paul F. Donald, Fiona J. Sanderson, Ian J. Burfield, Stijn M. Bierman, Richard D. Gregory, Zoltan Waliczky**

Rodríguez-Muñoz *et al.* raise concerns about our study on the effectiveness of the European Union's Birds Directive, based on the provenance of the trend data used and on the predictions that were tested. Here, we show that our results are robust to different assumptions of uncertainty surrounding trend estimates and that criticisms of the methods stem largely from misunderstandings of the original paper.

Full text at www.sciencemag.org/cgi/content/full/319/5866/1042c

# Soil Erosion: A Carbon Sink or Source?

Rattan Lal and David Pimentel

*Science* **319** (5866), 1040-1042.
DOI: 10.1126/science.319.5866.1040