LETTERS

edited by Jennifer Sills

Sea-Level Rise by 2100

IN HIS NEWS AND ANALYSIS PIECE REPORTING ON THE NEWLY RELEASED FIFTH ASSESSMENT report (AR5) by Working Group I (WGI) of the Intergovernmental Panel on Climate Change (IPCC) ("A Stronger IPCC Report," 4 October, p. 23), R. A. Kerr highlights three fundamental conclusions about climate change that were assessed with equal or greater confidence than in previous IPCC reports. He also points to three "contentious points" on which he states that the AR5 "took a moderate line." Kerr includes sea-level projections among these points, and reports "a rise of 40 to 60 centimeters by late in the century and a worst case of 1 meter by 2100, [which is] higher than in 2007 but far below the meter or two of sea-level rise that some expect."

As the authors of the IPCC WGI AR5 chapter on "Sea-Level Change," we wish to clarify that for the highest emission scenario considered (RCP8.5), the AR5 reported a "likely" range of 0.45 to 0.82 m for sea-level projections for the late 21st century (average over 2081 to 2100) and of 0.52 to 0.98 m by 2100. The difference in sea level between these two periods is large because in 2081 to 2100, the "likely" rate of rise is 8 to 16 mm per year, which is up to about 10 times the average rate of rise during the 20th century.

In the calibrated uncertainty language of the IPCC, this assessed likelihood means that there is roughly a one-third probability that sea-level rise by 2100 may lie outside the "likely" range. That is, the AR5 did not exclude the possibility of higher sea levels. However, we concluded that sea levels substantially higher than the "likely" range would only occur in the 21st century if the sections of the Antarctic ice sheet that have bases below sea level were to collapse. We determined with medium confidence that "this additional contribution would not exceed several 10ths of a meter of sea-level rise during the 21st century." We could not define this possible contribution more precisely because "there is currently insufficient evidence to evaluate the probability of specific levels above the assessed 'likely' range."

The upper boundary of the AR5 "likely" range should not be misconstrued as a worst-case upper limit, as was done in Kerr's story as well as elsewhere in the media and blogosphere. For policy and planning purposes, it may be necessary to adopt particular numbers as an upper limit, but according to our assessment, the current state of scientific knowledge cannot give a precise guide.

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Credit for Impact Theory

IN THE NEWS FOCUS STORY "IMPACT THEORY gets whacked" (11 October, p. 183), D. Clery summarizes the present conundrum we face in understanding the Moon's eerie isotopic similarity to the Earth. However, it contains one oversight in ascribing the proposal of the giant impact hypothesis to William Hartmann and Donald Davis (*1*) in 1975. In actuality, two groups developed this idea contemporaneously and both discussed essentially the same idea at a Cornell conference in 1974, at which all four researchers were present. Hartmann and Davis directly acknowledged this in a footnote in their paper.

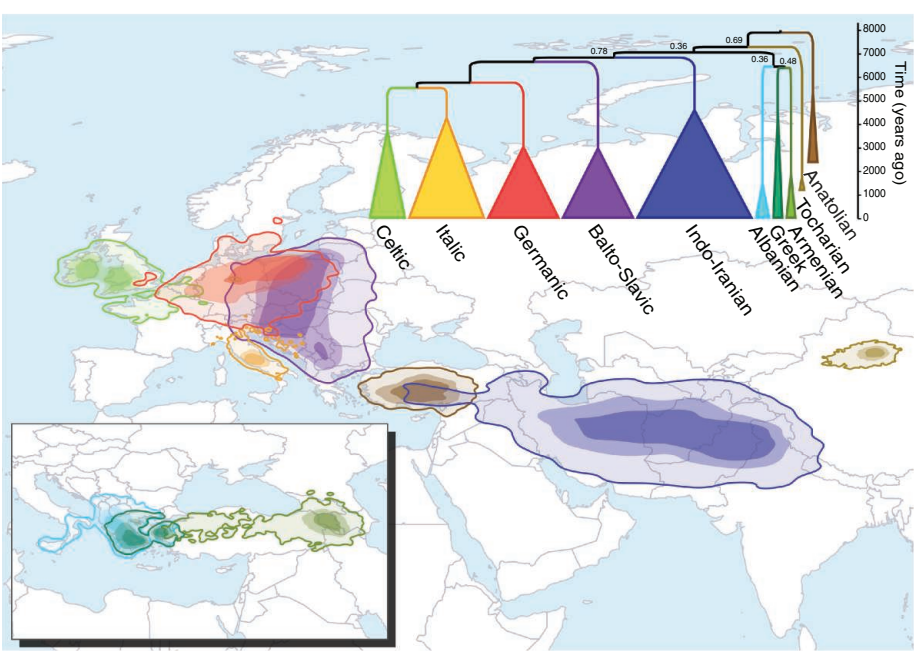
CORRECTIONS AND CLARIFICATIONS

Reports: "Anthropogenic seismicity rates and operational parameters at the Salton Sea geothermal field" by E. E. Brodsky and L. J. Lajoie (2 August, p. 543, published online 11 July 2013). There are two typographical errors in Table 1: The reported phase lag in the time interval of 1982–1991 associated with injection should be 0 instead of 6. Also, the correlation between injection and seismicity in the 1991–2006 time window should be 0.25 instead of 0.26. The HTML and PDF versions online have been corrected.

Reports: "Mapping the origins and expansion of the Indo-European language family" by R. Bouckaert *et al*. (24 August 2012, p. 957). The authors are grateful to William Chang and Andrew Garrett for informing them that there was a problem with the data matrix they used. The error occurred when 13 languages were removed from the original 116-language data matrix (http://ielex.mpi.nl) because they were colonial varieties or doculects, for which the authors had a better source. Removing these languages produced 283 "empty" columns of zeros (out of 6279), which the authors neglected to omit. Columns full of zero entries can potentially bias rate estimates from model-based phylogenetic inference. In addition, this revealed an error in the ascertainment bias correction for all-zero columns in the BEAST code [A. J. Drummond, A. Rambaut, *BMC Evol. Biol.* **7**, 214 (2007)]. The authors have therefore rerun the analyses with corrected data and BEAST code. The covarion model is now the best-fitting model of cognate evolution [C. Tuffley, M. A. Steel, *Math. Biosci.* **147**, 63 (1998); D. Penny *et al*., *J. Mol. Evol.* **53**, 711 (2001)]. Under this model, the basic inference about the geographic origins of Indo-European remains unchanged (revised Table 1 shown below); however, the tree topology differs slightly (revised Fig. 2 shown below) and date estimates are younger, although still showing a better fit with the Anatolian hypothesis than with the Pontic steppe hypothesis (median = 7579 years BP; 95% HPD interval = 5972 to 9351 years BP). The date ranges under the different models of cognate evolution, including the previously best-fitting model (the stochastic-Dollo), are shown in a newly added fig. S13. Revised supplementary material with revised versions of all affected tables and figures, as well as updated xml code, has been posted online. Two points in the main text also need correcting. First, the analysis, in which the authors constrain the tree topology to fit with an alternative pattern of diversification, still shows strong support for an Anatolian origin, but the Bayes factors are slightly different ($BF_{Steppe\ I}$ = 174.02, $BF_{Steppe\ II}$ = 145.35). Second, in the revised analysis, the five major Indo-European subfamilies—Celtic, Germanic, Italic, Balto-Slavic, and Indo-Iranian—all emerged as distinct lineages between 4000 and 7000 years ago, not between 4000 and 6000 years ago as previously stated.

Phylogeographic analysis	Bayes factor	
	Anatolian vs. steppe I	Anatolian vs. steppe II
RRW: All languages	380.4	625.2
RRW: Constrained	174.0	145.4
RRW: Ancient only	828	+∞
RRW: Contemporary only*	73	+∞
Landscape aware: Diffusion	161.10	79.14
Landscape aware: Migration from land into water less likely than from land to land by a factor of 10	63.0	31.2
Landscape aware: Migration from land into water less likely than from land to land by a factor of 100	120.3	59.0
Landscape aware: Sailor	119.4	59.6

*We note that although this analysis appears to show strong support for the Anatolian theory, this is because the Kurgan homeland was never sampled, whereas a small number of samples fell within the Anatolian range. This is perhaps not surprising given the absence of Anatolian and Tocharian languages from this analysis.



Hartmann and Davis were first to publish. They advocated that Earth collided with a sublunar mass object near the end of its formation, based on models of the Earth's assembly. If the Earth's core had formed at the time of the collision, they argued, ejected material would be depleted in iron, thus offering a natural explanation for the Moon's low density. Cameron and Ward's work appeared in early 1976 (*2*). They recognized two additional (and critical) aspects of the problem: First, a mechanism to alter ballistic trajectories (such as vaporization and resulting pressure gradients) would be required to allow ejected material to go into orbit around the Earth. Second, the impact scenario implies a Mars-sized impactor, based on matching the anomalously large angular momentum of the Earth-Moon system.

Given this, most of the Moon origin community attributes the impact theory jointly to both Hartmann and Davis (1975) and Cameron and Ward (1976).

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Social Security and Medicare in the Black

IN HIS NEWS & ANALYSIS STORY, "U.S. SHUTdown ends, but not budget anxiety" (25 October, p. 410), J. Mervis writes, "Hunter Rawlings, president of the…Association of American Universities,…says one major impediment to increased science spending is the continued growth of so-called entitlement programs, such as Social Security and Medicare." Social Security is paid for by a tax dedicated to Social Security. Medicare Parts A and B are also paid for by a tax dedi-

Letters to the Editor

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cated to Medicare. Although future problems loom, to date both programs have run in the black through the course of their history (*1*). In other words, neither Social Security nor Parts A and B of Medicare have contributed a penny toward the current U. S. deficit. On the contrary, the discretionary side of the budget has borrowed heavily from them.

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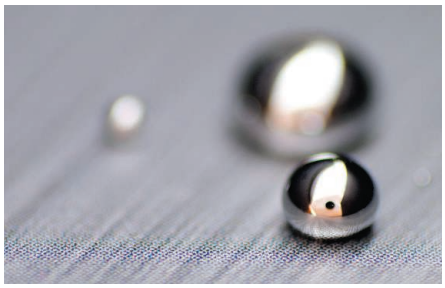
Mercury Toxicity in Children



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AS SCIENTISTS AND PEDIATRICIANS WHO study the impacts of toxic chemicals on children's health and deal with the consequences of exposure, we are concerned that in her Editorial "Mercury and health" (27 September, p. 1430), M. McNutt suggests that more research is needed to determine the effects of sublethal doses of mercury on the development of young children.

The developmental toxicity of mercury has been studied extensively for more than two decades (*1*–*10*). A major review by the National Academy of Sciences (*1*) concluded that evidence for the developmental neurotoxicity of methylmercury is strong and highly credible, even at low levels of exposure. These findings provide critical support for the Minamata Convention. The Editorial's call for additional research, at a time when abundant scientific evidence already exists, may delay full ratification of the Minamata Convention. Unnecessary calls for additional research have been a major factor in long delays in recognition, remediation, and reparations to the victims at Minamata.

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