The last example I want to discuss is that of Nina Federoff's development (and transformation) of Barbara McClintock's work on genetic transposition. Here she suggests there are two gaps to be filled: first, as in the case of Berzelius' notation, between the microscopic (the level of DNA) and the macroscopic (the visible patterns on the corn kernels); and second, between classical genetics and molecular biology. Because I am on somewhat surer ground here, I will take the liberty of suggesting some additions to rather than simply summarizing Emily's account. For example, I would like to add a third gap, perhaps especially relevant to an analysis of representations, namely the gap between Federoff's account of this work, conveyed (I assume) within the last decade, and McClintock's own account, conveyed in the late 1970's. (As an aside, I might also add that there is an interesting polyvalence (or ambiguity) in the history McClintock herself – an ambiguity that has given rise to a certain amount of controversy, but that should no longer disturb us here. My own account of McClintock as an underappreciated maverick has been held up as contradicting accounts that emphasize both her obvious success by many measures and the enormous regard with which she was held by many scientists. The fact is, both are true…)

But to more important issues, namely the matter of transposition itself, a phenomenon that, before the late 1970's, few people believed in, despite their enormous regard for McClintock. What, Emily asks, turned them around? Her answer may overemphasize the role that Federoff's own work played, but there is no doubt that the acceptance of the fact of transposition in the 1980's depended on its being documented by evidence from molecular biology. Indeed, by 1980, not many biologists with the necessary expertise in classical genetics were still around. Acceptance was up to a new audience of readers, with techniques that opened up an entirely new domain of investigation not accessible to classical geneticists, namely, the microscopic domain of nucleotide sequences.

Emily writes, "Federoff's molecular biology … was able to answer certain questions that McClintock's genetics could formulate, but not fully investigate." I would suggest, rather, that MB was abler to answer certain questions that McClintock did not ask, but that a later generation would find necessary to ask – especially, questions about the mechanism by which transposition was effected, where the notion of "mechanism" had taken on a very particular meaning. (E.g., no physicist or engineer would accept the currently available account as a 'mechanism'.) For McClintock, belief in transposition did not depend on knowing the means by which it occurred: proof of its existence lay elsewhere – in the observations that were available to her. My point is that, even assuming that Federoff and McClintock were investigating the same phenomenon, they did so with different questions, and with different aims, and some of these differences make going back and forth between different representations, or different readings of the same representation, exceedingly difficult. On the question, e.g., of the importance of transposition to genetic regulation, Emily is certainly right in saying the jury is still out. But the distinction between transposition and regulation that is so clear to her somewhat escapes me, and I think would have mystified McClintock: Moving genetic elements around has effects that may or may not be regulatory, but that distinction has nothing to do with the molecular mechanism by which the transfer of sequences occurs; rather, it refers to the relation between sequence structure, transcription, translation, and all the other higher order processes requiring regulation. And one of the reasons the jury is still out about the role of transposition in regulation is that molecular biology is just beginning to unravel the enormous complexity of regulatory dynamics.

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