

Why was Galton so concerned about “regression to the mean”? A contribution to interpreting and changing science and society

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This essay interprets Francis Galton’s explicit concerns about biological inheritance in terms of his implicit views of the appropriate role of his social stratum. The pattern of regression means that exceptional individuals in one generation cannot rely on biological heredity to guarantee that their offspring will be part of the next generation’s exceptional individuals. Interpretation of Galton’s concerns with regression points to social and scientific issues that still require examination and clarification: confusions about the relationship between meritocracy and heredity; the persistence of typological thinking; and how directly we expect patterns in data to provide insight about processes. An initial classroom activity establishes the pedagogical tone for the essay, with its emphasis on fundamental concepts accessible to non-specialists and on motivating themes that open up inquiry into complexity. The essay as a whole exemplifies and promotes more self-conscious attention to the ways that our interpretations in and of science are intended to influence change in science and in society.

Introduction: Interpreting and changing science and society

The young Karl Marx proclaimed that the “philosophers have only *interpreted* the world, in various ways; the point, however, is to *change* it.” But what mode of interpretation should guide people in effecting change? That’s no simple matter—Marx himself spent the following forty years of his life elaborating his interpretation of historical and ongoing social transformations.

An English contemporary of Marx, Francis Galton, sought to promote social progress by interpreting patterns in data drawn from human relatives. As Galton proclaimed early in the forty years of research he conducted:

If a twentieth part of the cost and pains were spent in measures for the improvement of the human race that are spent in the improvement of the breed of horses and cattle, what a galaxy of genius might we not create! ...Men and women of the present day are, to those we might hope to bring into existence, what the pariah dogs of the streets of an Eastern

town are to our own highly-bred varieties (Galton 1865, 165-6).

The motivation for this essay is also a concern with the relationship of interpretation to change, but in a sense more modest than in Marx or Galton’s visions of revolutionary social transformation. For some time I have been trying to get a better handle on ways that interpretation of particular episodes or strands of science can contribute to change in the science and its applications of an ideological or practical nature (Taylor 2005). This has led me to pay attention to, among other things, the tension between, on one hand, accounts built around simple themes that are readily conveyed and digested by a wide audience and, on the other hand, accounts of the particular complexity of the diversity of things scientists do and the diversity of resources they use in the process of making science. I will return to the simple side shortly. On the complexity side, the social studies of science has since the 1980s highlighted the ways that scientists employ or “mobilize” equipment, experimental protocols,

citations, the support of colleagues, the reputations of laboratories, metaphors, rhetorical devices, publicity, funding, and so on (Latour 1987, Law 1987, Clarke and Fujimura 1992, Clarke 2005, Taylor 2005). My personal experience pursuing quantitative research matches this picture and has led me to explore the idea that anyone wanting to influence developments in some area of science would benefit having some kind of “map” of the complexity of resources or practical commitments involved in knowledge construction in that area. Such maps point to multiple places at which concrete alternative resources could be mobilized, which allows a range of researchers, taking into account their own background and interests, to identify specific changes that they could effect (Taylor 2005). In this way, change in science can be guided by interpretations of the diversity of things scientists do in practice.

A consequence of this kind of interpretation is that each case of science-in-process has its own idiosyncratic complexity. Examining this may be stimulating to some group of specialists interested in the particularities of the given case, but, in order to engage a wider audience, a complementary approach is called for. Suppose that a teacher or writer still wants—as I do—students or readers to delve into the practical and intellectual complexities of particular areas of scientific activity that they are engaged with. One might sidestep the enormous task of providing maps of the complexity for multiple cases that span the interests of the audience, and instead aim to guide or inform the choices that students and readers go on to make as they shape their own paths into that complexity. One might even just aim to stimulate students and readers to think about aspects of complexity they had not given much or any attention to before. In that spirit, one pedagogical-expository approach I use is to try to motivate simple themes that, although readily conveyed and digested, at the same time open up questions and point to further work

needed to grapple with the complexities in particular cases (Taylor 2005). These themes are not meant as *lessons* or *knowledge claims* (even in the form of ideal types or abstract generalizations) that might empower readers or students who accept them. The intended relationship of such “opening up” themes to change is more modest: they add to the “tool box” of ideas that students or readers draw from to assemble a response to any new situation in its typical complexity.

This is the pedagogical-expository approach I adopt through the three parts of this essay. As the title indicates, I interpret an aspect of Galton's work. Before doing so, however, I set the scene with a classroom activity that establishes the pedagogical tone for the essay, with an emphasis on fundamental concepts accessible to non-specialists and on motivating themes that open up inquiry into complexity (sect. I). Then I develop an interpretation of Galton's interpretation of a pattern he called regression (sect. II). I link Galton's explicit concerns about *biological inheritance* to implicit views of the *appropriate role of his social stratum* and in so doing answer the essay's title question quite differently from previous interpreters who link Galton's work on regression to its social and intellectual context. Others have either focused on his general interest in a science of human heredity (MacKenzie 1981, Porter 1986) or accounted for Galton's specific concerns about regression in ways that, as will become evident, leave interesting questions unanswered (Provine 1971, Bowler 1984, Kevles 1985, and Gayon 1998). However, although this essay opens up areas for further historical research, it is more a conceptual than a historical contribution. In that spirit, I go on to argue that interpretation of Galton's concerns with regression *as he first invented it* points to social and scientific issues that still require examination and clarification (sect. III). We should not discount Galton's concerns just because the term regression is now used to refer to statistical methods more varied

and mathematically sophisticated than Galton could have dealt with.

All three parts of the essay are intended to promote (citing here this journal's aims) "a critical perspectives [on] statistical concepts, methods, and practices," probe "the implicit worldview of professional statisticians," and contribute to "social and scientific criticism." Yet "criticism" has ambiguous connotations, both of interpreting and of changing. It is with this in mind that the three parts are combined in a single essay, building on each other so as to contribute to more self-conscious attention to the ways that our interpretations in and of science are intended to influence change in science and in society.

In the spirit of pedagogical or expository self-consciousness, let me identify at the outset three "opening up" themes that underlie the essay. The first has been articulated in this Introduction; the second concerns the relationship of interpretation to change; and the third will be evident shortly. (Extensions of these themes will be spelled out at the end of each section.)

Theme 1:

Interpreters of science can approach the complexity of particular cases indirectly by motivating simple themes that open up questions and point to further work needed to grapple with the complexities of other, particular cases.

Theme 2:

It can be illuminating to ask what the authors (including ourselves) state or imply about what we can do. (This deliberately broad formulation encompasses views about the social actions and organization they support as well as their views about the capabilities of different people growing up in our

society and how difficult these are to change.)

Theme 3:

Close examination of concepts and methods within any given natural or social science can stimulate our inquiries into the diverse social influences shaping that science, and reciprocally.

I. Patterns among relatives:

A classroom activity

Before I discuss Galton and regression, let me introduce a classroom activity that establishes a pedagogical tone for the essay and an emphasis on fundamental concepts. I want to ask you, the reader, to be a scientist and try to make sense of data that link parents and offspring. Consider one plot (of your own choosing) from figures 1-4 depicting heights of 63 undergraduate college students and their parents. (I collected these data in the USA in the mid to late 1990s.) What patterns can you discern? What ideas or questions do you have about the causes producing those patterns? What questions or reservations do you have about the process you go through in answering these questions?

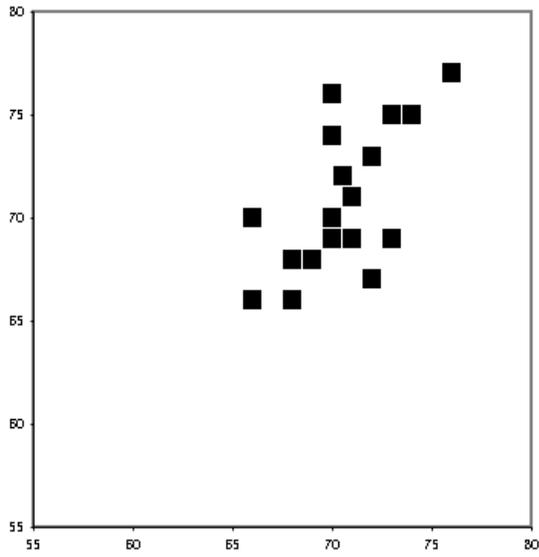


Figure 1:
Son's vs. father's height (inches)

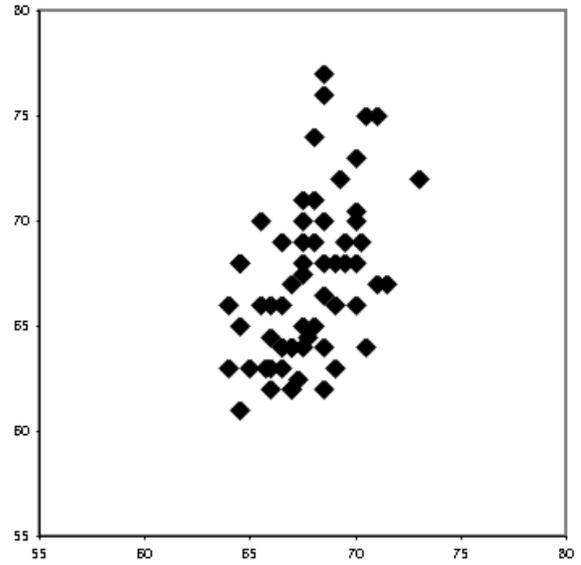


Figure 3:
Student's vs. average of parents' height (inches)

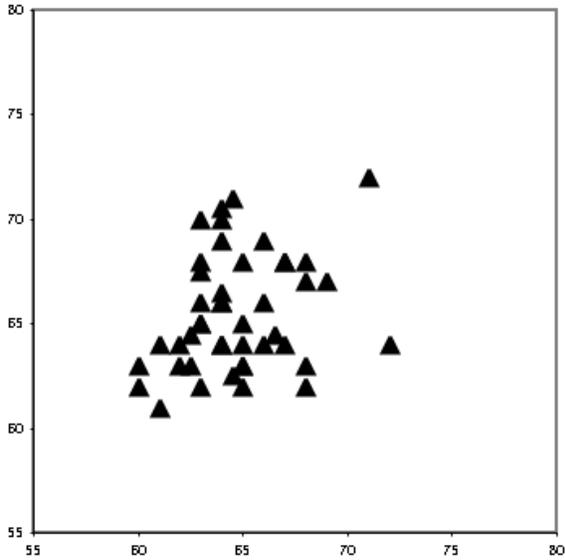


Figure 2:
Daughter's vs. mother's height (inches)

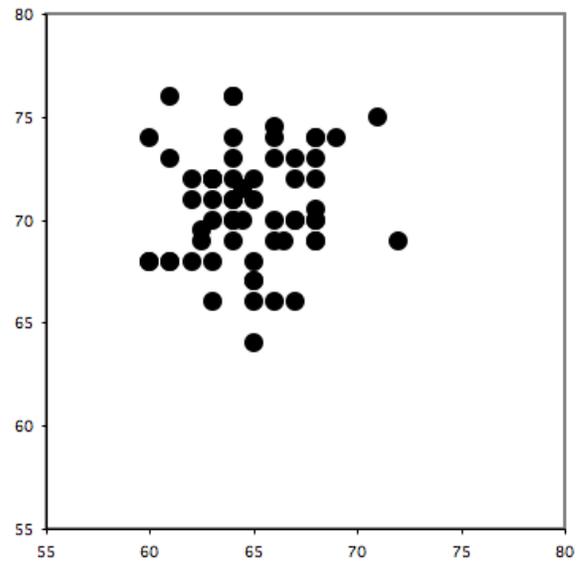


Figure 4:
Father's vs. mother's height (inches)

OK readers. Keep your answers in mind as I describe what usually emerges when I ask these questions in my classes on biology and society. Students identify patterns in many ways. They draw boxes, ellipses, or convoluted shapes around the data points, mark highs and low values for each of the variables, note how many offspring are taller than their parents, separate the main cloud of points from outliers, draw trend lines through the cloud, and so on. Many students note that in the first three plots an increase in one variable tends to be associated with an increase in the other (albeit with considerable spread around any trend line). No trend, however, is seen in figure 4, which depicts the heights of each pair of parents. Indeed, often students will say there is no pattern in that plot. Some students notice the outlier half way up on the right in which the mother, at 72", is 3" taller than the father. They do not notice the pattern that the father is taller than the mother in almost every pair, but see it once I point it out.

When it comes to explanation, the first three plots are typically seen as indications of the hereditary relation between parents and offspring. Because there is no hereditary relation between any mother and father, students conclude at first that no causality can be drawn from figure 4. However, once I have drawn attention to the strong father-taller-than-mother pattern, lively discussion about the causes ensues for this plot too: Does this pattern correspond to men choosing female partners shorter than them or to women choosing male partners taller than them? Or both?

A range of questions or reservations are expressed about the process of this scientific inquiry, including the reliability of the data (how accurate are the data, which presumably came from students' recall or phone calls to their parents); criteria for inclusion (could adoptive or step-parents have been included); whether the students have stopped growing (perhaps heights should have been collected for parents when at the same age as their child is now); and

whether outliers warrant special explanation (or can they be viewed as points at the end of a spectrum).

As the teacher I inject further issues of critical thinking into the discussion: What additional knowledge leads the students to invoke heredity? (Couldn't height trends result from parents feeding their children the way they were fed?) Why plot same sex pairs and exclude the opposite sex parent? (Is this a choice dictated only by the difficulties of plotting in three dimensions?) Why plot offspring height against the average of the parents? (Does this presume that height is a blending of contributions—hereditary or otherwise—from parents?) Most importantly, what could anyone do (or be constrained from doing) on the basis of the patterns or explanations?

On this last issue of "what can we do?", I note that the mother-father height pattern, originally overlooked by students, is of great significance to taller heterosexual women because it corresponds to a smaller selection of men available to them as potential partners. If the height norm were contested, these women would have new options opened up. It would also reduce the frequency of couples in which the man is very much taller and stronger than the woman. In contrast, the hereditary explanation of the trend in the first three plots does not suggest any action other than inaction—parents cannot do anything to change the outcomes for their offspring once these offspring have been conceived. This inaction conclusion about height might not trouble us, at least not enough to make us delve into possible relationships between growth trajectories and, say, maternal nutrition before and during pregnancy, childhood diet, exercise, and so on. However, I ask my students, if the data were of IQ test scores, not heights, would inaction be an acceptable conclusion? Or would they pursue the process of identifying patterns, proposing explanations, exploring reservations (including raising alternatives) differently?

This simple classroom activity allows us to unpack the simple picture of science as empirical observation and rational interpretation (i.e., identifying patterns and trying to explain them). These are only two of the many steps in scientific inquiry (figure 5). At each step decisions are made that depend on knowledge—perhaps assumed knowledge—in addition to what can be drawn from any data collected. Scientific inquiry cannot proceed without decisions that take into account diverse additional considerations, such as, in this classroom activity: technical constraints of plotting in three dimensions; theories about the mechanisms of heredity, temporal ordering (parents grow before their offspring are born and grow), whether to collect data about the diet of parents and offspring when they were

growing up, and conventions about designation of outlier status to extreme points. Each step becomes a site where decisions made can be shaped by convention, ongoing negotiation, and wider influences. These “sites of sociality” invite critical scrutiny (Taylor 2005). We can, for example, consider the ways that preconceptions or preferences about the outcomes at the later steps feed forward to earlier ones (as depicted by the dashed lines in figure 5) so that the inquiry tends to reinforce that outcome. As will be shown in the discussion of Galton’s work, such feed forward loops can involve the social actions or organization supported or desired by scientists—what they think we as a society can or should do.

All possible phenomena that could be inquired into

(-> subset of phenomena generated by experimental manipulation)

-> phenomenon deemed interesting for study

-> questions asked about the phenomenon

-> categories demarcated in the questions

-> observations made within those categories

-> data collected from the observations

-> patterns perceived in data

-> predictions made based on the patterns or hypotheses about causes

-> actions supported by predictions or causes

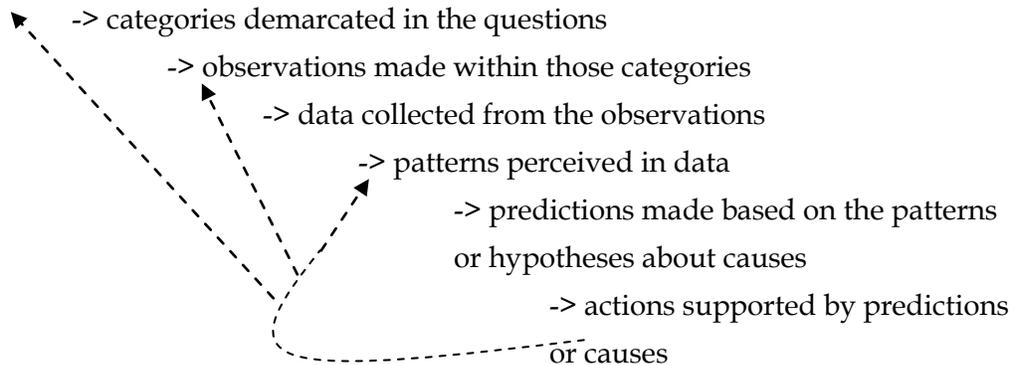


Figure 5
A chain of steps in scientific inquiry

A chain of steps in scientific inquiry in which each step (indicated by an arrow ->) involves assumptions and is open for negotiation and wider influences. The dashed lines depict the possibility that desired outcomes for the later stages influence decisions made at earlier steps. See text for discussion.

Through this classroom activity extensions of themes 2 and 3 have emerged:

Theme 4:

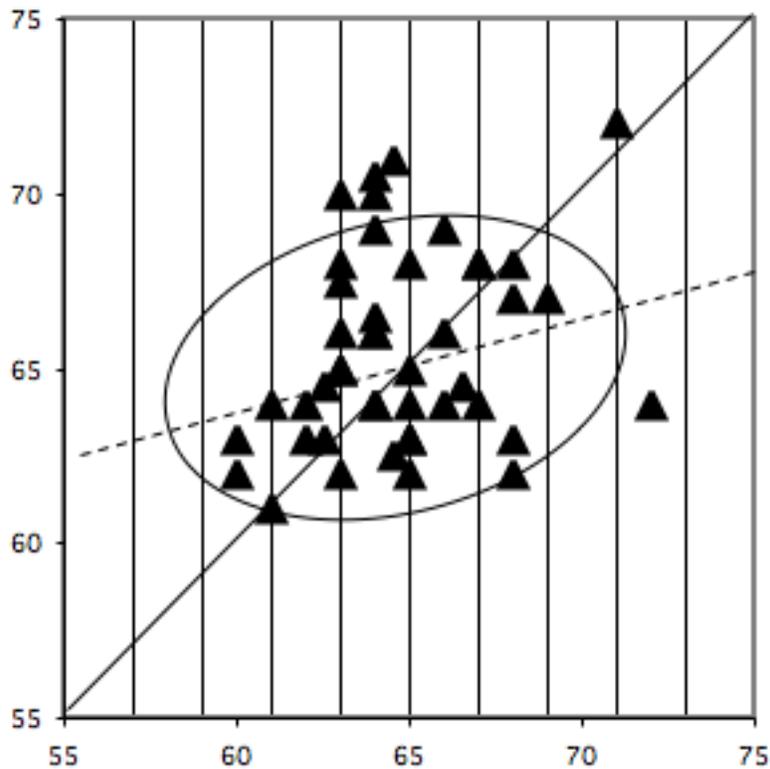
There are many sites in scientific inquiry at which decisions are made based on knowledge drawn from outside the observations to be explained.

Theme 5:

The negotiation, assumptions about social possibilities and constraints, and wider influences that shape decisions made at these open sites invite critical scrutiny.

II. Why was Galton so concerned about “regression to the mean”?

Students in introductory statistics courses learn that correlation is a measure of the tightness of association between measurements of different attributes or variables for each individual in some set. For example, for each student in the previous section I had recorded the height of the student, the student’s father, and the student’s mother. If for each male student, the height were tightly associated with the height of their father, a plot of the two measurements would fall close to a straight line. Because the correlation is looser the plot is a cloud of points with some tendency upwards from left to right (Figure 1).



**Figure 6:
Daughter's vs. mother's height (inches)**

Solid line denotes equal values. Dashed line is the regression line. The mean is the point where the two lines cross. The ellipse approximates the cloud of points (as discussed later in the text).

Statistics students also learn about regression lines, which can be drawn through the cloud to give the best prediction of one measurement (e.g., daughter's height) on the basis of the other measurement (e.g., mother's height). "Best" here means that the discrepancies between the actual and predicted values are minimized (strictly, the average of the discrepancies squared are minimized). This can be seen by eye by marking the average daughters' height in each vertical slice in figure 6. The regression line does not run right through the averages but finds an overall balance of averages above the line with those below. (Present-day forms of regression analysis can be more complicated, involving curves through the data and minimizing discrepancies in other ways, but the simple, "linear" regression was the original sense and the basis of Galton's concerns. It is the sense used throughout this essay.)

Correlation and (linear) regression lines capture the same quality of the measurements, namely, the tightness of clumping around a line. Indeed, if the plot is scaled so that the spread of each variable is the same (strictly, if the standard deviations are equal), then the slope of the regression line is exactly the same value as the correlation (Weldon 2000). To understand the significance of this overlap and the strange name "regression," we need to look at its origins in the nineteenth-century work of Galton.

Francis Galton, a younger cousin of Charles Darwin, introduced both concepts — regression first in the late 1870s and then correlation in the late 1880s—as he investigated similarities among relatives, especially parent and offspring pairs, for an enormous variety of measurements, primarily of humans—from height to mental traits, such as perception of numbers as colors. Through this work Galton became a leading figure in the rise during the nineteenth century of quantitative science of society that sought regularities or laws in the statistics collected by the growing bureaucracies of the nation state—as well as by assiduous

individual data collectors like Galton himself (Porter 1986).

Galton's investigations led him to note that "the progeny of all exceptional individuals tends to 'revert' to mediocrity" (Galton 1877, 283). (Subsequently Galton would replace "reversion" with the term "regression.") He concluded that "the ordinary genealogical course of a race [today we would say "population"] consists in a constant outgrowth from its center, a constant dying away at its margins, and a tendency of the scanty remnants of all exceptional stock to revert to that mediocrity, whence the majority of their ancestors originally sprang" (1877, 298; hereon, citations without an author's name are to Galton's publications).

Clearly Galton was concerned about human progress—or obstacles to it. Indeed, his investigations originated in his desire to extend to humans the investigation of selective breeding of plants and animals, which Darwin (1859) had used to motivate many features of his account of natural selection. Uncovering the laws of human heredity was essential for this project of science-based social improvement (MacKenzie 1981, Porter 1986). Galton's work on measurement, analysis of association among relatives, and guided evolution of humans has led to his being seen as the founder of several fields— psychological testing, biometry (statistical analysis in biology), behavioral genetics, and eugenics. In the eugenic context, Galton noted that, because measurements of the offspring of the exceptional regress towards the center—or mean—of the range of measurements, "it is... impossible that the natural qualities of a race may be permanently changed through the action of selection on mere variations" (1892a, xviii; see Waller 2001 for a more qualified account of Galton as eugenics' founder).

The statistics student of today learns to calculate correlations and derive regression lines for data sets that do not involve heredity or improvement over time, for example, data

linking the number of rooms in houses and their selling price. So why was the originator of these statistical concepts so concerned about regression to mediocrity, or, as statisticians now phrase it, “regression to the mean”? The answer I arrive at in due course is going to refer not only to Galton’s explicit interest in human betterment, but also to his *implicit* ideas about what people can do to pursue their interests. In order to motivate my answer, I will move through a series of other possible answers first and consider an allied question about Galton’s views on selection.

As noted above, similar measurements among relatives for a given trait say nothing on their own to distinguish, in Galton’s words, “between the effects of tendencies received at birth, and of those that were imposed by the circumstances of their after lives” (1875, 566). However, especially for the traits that concerned him, namely, “superior faculties” or abilities that were “exceptionally high” (1892a, viii), Galton was convinced at an early stage of his inquiries that “nature prevails enormously over nurture” (1875, 574). To Galton this was evident in the biographical data he had collected on illustrious men and their kinfolk (1869) and in studies of the life histories of similar and dissimilar twins (1875). His conclusion about nature over nurture may be less than convincing to us—at one point he begged the question by defining the traits he was measuring as those that “exclude the effects of education” (1892a, viii). What is pertinent, however, is that his conclusion meant he saw no need for data on what we would call environmental or social variables. He could investigate heredity through the patterns of similarity among relatives. Regression was one of those patterns.

Now, does regression to the mean result literally in regression *to* the mean? Do measurements of individuals in each succeeding generation pack ever more tightly around the mean? Commonsense says no and, indeed, in places Galton acknowledged this fact. He wrote of a “constant outgrowth from [the] center”

(1877, 298) and in *Natural Inheritance* (1889), which synthesizes his investigations of quantitative studies of heredity, he noted that “the observed proportions between the large and the small in each degree of size and in every quality, hardly varies from one generation to another” (1889, 2).

When viewed over many generations there is no trend for measurements to regress literally to the mean. Logically, therefore, over a single generation there should be no regression towards the mean. Yet, notice that regression does seem to hold for offspring of “exceptional” individuals. For example, in figure 6, the daughters whose mother’s height is in the lowest “slice” are all between the mean and the solid line that has slope 1. In other words, the daughters are not so far away from the mean as their mothers. The one offspring in the highest slice is also closer to the mean (albeit on the other side). (Recall that “slope of 1,” “closer” and other references to distance assume, as is the case in Figure 6, that distance is scaled so that the spread [the standard deviations] of the two variables are equal.) Is there a paradox here?

Let us look at what holds for less-than-exceptional individuals (which was not a component of the population Galton dwelled on apart from his reference to a “constant outgrowth from the center.”) Notice that for slices further in from the extremes in Figure 6, some daughters are closer to their mean than their mothers are, but some are further away. What is true is only that the *average* of the offspring is closer to the mean. We can also see that some offspring are on the other side of the mean. For the most central slices, this becomes even more noticeable; indeed, many of these offspring are further away from the mean *but on the other side*. In fact, if these *digressions* from the mean on the other side did not occur, it would be logically impossible for the average of offspring to be closer to the mean while preserving the same spread from one generation to the next. Once this point about digressions is

appreciated, it becomes clear that for *any two correlated variables*, individuals whose measurement on the first variable is a certain distance from the mean for that variable will show a range of measurements on the second variable and that range will have an average value closer to the mean for that second variable. No hereditary relatedness is required for this pattern to occur. (Indeed, the variables could be switched and the same pattern would be evident.)

We can still call the parent-offspring pattern for less-than-exceptional parents “regression” provided we note that it is regression of the *average* of the offspring towards the mean where many of these offspring will be *further* away from the mean. Galton’s choice of the term “regression” clearly stemmed from his focus on exceptional individuals (where few offspring are further from the mean), not on the population (“race”) as a whole. OK, but this deepens the puzzle: if the regression of the offspring of exceptional individuals towards the mean does not entail the population collapsing to the mean over many generations, why was Galton concerned about regression towards the mean? After all, without any decrease in variation from one generation to the next, there will be no shortage of persons who are most “efficient in physical, intellectual, and moral grounds” to form “our highest [social] class” (1892a, xxii).

One possible answer, given by historians of science Bowler (1984) and Kevles (1985), follows from Galton’s conclusion (1892a, xviii), cited earlier, that regression ensures that “selection on mere variations” cannot produce any permanent improvement in a population. As Bowler (1984, 240) explains it:

Imagine a sample of individuals from a particular part of the range of variation, such as a group of people with above average height. What will happen to the sample if we allow the individuals to breed only among themselves for a series of generations? Galton believed that the

mean value of the characteristic for the sample would regress back toward that of the species as a whole. After a number of generations, descendants of our sample of tall people would have an average height equivalent to the normal for the human race.

Or, in Galton’s own words from an early point in his investigations (also cited earlier), “the scanty remnants of all exceptional stock... revert to that of mediocrity, whence the majority of their ancestors originally sprang” (1877, 298).

There are problems, however, in accounting for Galton’s concern about regression in terms of the purported ineffectiveness of selection. It is not true that an above-average sample allowed to breed within itself must end up no longer above the average. Galton seemed to recognize this. Although he did not explore the theory or practice of breeding within a selected sample, he acknowledged the possibility that advances could be made. In his preface to the 1892 reprint of his 1869 book, he cited the “Huguenots as men, who, on the whole, had inborn qualities of a distinctive kind from the majority of their countrymen, and who [are] capable, when isolated, of continuing their race without its showing any strong tendency to revert to the form of the earlier type from which it was a well-defined departure” (1892a, xxiii).

The puzzle is now more complicated. To the question of why Galton was concerned about regression towards the mean, we have to add: Why was Galton confused—or, at best, confusing—about whether a selected sample would regress to the mean of the population (which is not correct) or could maintain its distinctive qualities if allowed to breed within itself? Why did he not follow through the logic of the latter possibility after he acknowledged it? As became clear to evolutionary biologists in the twentieth century, selection of a sample, then a sample of this sample, and so on, results in continuing change over time (as long as variation remains and inbreeding is small

enough to keep deleterious genes masked). Let me put forward four interpretations of Galton's erroneous interpretation of the ineffectiveness of "selection on mere variations." This will lead us to a place where the title question can be answered.

The first interpretation builds on Galton's interest in human betterment. Selection of a sample of the human population was, Galton remarked in the 1892 preface, primarily of "academic interest." Admittedly he showed chilling prescience of twentieth-century "racial hygiene" in the passage that followed:

Thought and action move swiftly nowadays, and it is by no means impossible that a generation which has witnessed the exclusion of the Chinese race from customary privileges of settlers in two continents, and the deportation of the Hebrew population from a large portion of a third, may live to see analogous acts performed under sudden socialistic pressure (1892a, xx).

Nevertheless, the practical schemes he proposed did not rely on large scale, coercive controls over who reproduced. Instead, Galton's eugenics (a term he coined) consisted of appeals and incentives to encourage those of high rank to have more children—at the very least, not to have fewer children than the population at large. If Galton were (following theme 2) to ask, "What can we do?", "we" would refer to his peers of high social rank; it was conceivable to him that they could increase their contribution to the next generation. (See Waller 2001 for discussion of family versus national-level concerns about heredity in nineteenth-century England.)

The second interpretation is that Galton, for all his pioneering contributions to the analysis of variations, had not escaped from a typological worldview, in which variation is conceived in terms of deviations from a true value. Granted, when, as Stigler (1986, 265ff) describes, Galton sought to reconcile heredity with the ubiquitous bell-shaped distributions of

plant and animal traits, he was contesting the implication that such variation could be understood in terms of Quetelet's law of errors, in which the distribution "should be wholly due to the collective actions of a host of independent *petty* influences [or errors] in various combinations" (Galton 1877, 289). Notwithstanding this rejection of a view of deviation as error, Galton was conforming with a typological worldview when, addressing the perceived ineffectiveness of selection upon the typical range of variations, he emphasized the role of "sports" (i.e., mutant individuals), in which "a new character suddenly makes its appearance in a particular individual, causing him to differ distinctly from his parents and from others of his race." (The male subject is hardly remarkable for a nineteenth century writer, but it reminds us that the "we" who could produce human betterment were men.) Galton continued: "Such new characters are also found to be transmitted to descendants. Here there has been a change of typical centre... a real step forward has been made in the course of evolution" (1892a, xviii-xix). Moreover, "sports do not blend freely together; variations proper do" (1892b, 20).

The third interpretation is that evolution moving in discontinuous steps is an idea that persisted from the earliest days of Galton's inquiries following his reading of Darwin (as described by historian Provine 1971, 17). Like many other evolutionists until well into the twentieth century, Galton rejected Darwin's account of change through "insensible gradations" (1869, 42). He put forward an image of a stone of many facets that might be pushed continuously but would return to its original state unless that pressure took it beyond a threshold and it rolled onto a new face (1869, 421-2). Galton continued to develop analogies from geometry and human inventions to illustrate discontinuous change (e.g., 1889, 28ff) and asserted that "many, if not most breeds, have their origins in sports" (1894, 365), which are then "favored by Natural Selection" (1892b,

20) over other forms, just as “one race has supplanted another [with great frequency] in the evolution of mankind” (1892a, xxiii). This intellectual commitment of Galton’s leaves little room for conceptualizing evolution and human betterment that “proceed[s] by steps that are severally minute, and that become effective only through accumulation” (1889, 32).

A final interpretation builds on the previous one. It might be possible to attribute Galton’s emphasis on sports and his lack of interest in selection acting on the typical range of variation to this intellectual commitment to discontinuous change, one that pre-dated his investigations of similarities among relatives. It is also possible, however, to see the social action Galton favored (recall my themes 2-4) in his inability to clear up his confusion about whether the distinctive qualities of selected samples (or sub-populations) must regress towards the population mean or, as Galton saw in the case of the Huguenots, need not. In Galton’s view of society, exceptional individuals must have a role—or, at least, some exceptional individuals—in the betterment of a population. This, in turn, allows superior populations or races to supplant others. There *has to be something that the elite can do*.

I will not attempt to adjudicate between intellectual and social explanations of Galton’s confused or confusing position on regression and selection, but I am prepared to propose that social commitments are the key to explaining his concerns about regression. What the pattern of regression means is that the offspring of individuals at the extreme of the range for any given trait are, with rare exceptions, less extreme. What is not true is that this leads over a number of generations to the population losing its extreme values. Each generation will have its exceptional individuals, so the only consequence of regression towards the mean is that *exceptional individuals in one generation cannot rely on biological heredity to guarantee that their offspring will be part of the next generation’s exceptional individuals*. Galton’s investigations of

regression and correlation, therefore, provided no *biological* justification for elites to do something they are well able to do *socially*, namely, place one’s son “in a more favorable position for advancement, than if he has been the son of an ordinary person” (1865, 161). The pattern of regression, which occurs for any two correlated variables, is not by itself a problem when understanding or manipulating *biological* inheritance. However, in Galton’s project of human betterment, regression was a problem because it detracted from the case for *social* inheritance. This is my answer to the essay’s title question.

Such an answer shifts the focus from biological to social inheritance. To do so I have had to make reference to what is *implicit* in the logic of Galton’s work on regression (theme 3; Taylor 2006). The social interpretation does, however, find some *explicit* support. In the 1892 preface Galton states:

The question to be solved relates to the hereditary permanence of the several [social] classes. What proportion of each class is descended from parents who belong to the same class, and what proportion is descended from parents who belong to each of the other classes? Do these persons who have honourably succeeded in life, and who are presumably, on the whole, the most valuable portion of our human stock, contribute on the aggregate their fair share of posterity to the next generation? (1892a, xxii).

Nevertheless, let me concede that my interpretation lies on the simple formulation side of the Introduction’s simple-complex tension. It does not encompass the many particularities of Galton’s work (Stigler 1986, 265ff) and its social context (MacKenzie 1981, Porter 1986, Waller 2001). One might note, for example, that, although Galton’s financial independence resulted from a large inheritance, he took steps to factor out the effects of social

inheritance in his early biographical investigations of whether eminent men tend to have eminent relatives. As he explains, his focus on eminence in science and literature came from a choice to exclude the army and legislature because “neither... afford, in the highest ranks, an open arena to the ablest intellects” (1865, 161). In other words, the elites whose reproduction Galton was concerned with were not from the aristocratic upper class (who secured their position by means other than their intellect), but, as MacKenzie (1981) argues, were from the new professional middle class. (My interpretation has clear affinities with MacKenzie's, but he sees Galton's work on regression and correlation in terms of a general project of understanding the laws of heredity; MacKenzie does not delve into why Galton was specifically concerned about *regression to the mean*.)

At the same time, my simple formulation fits the intent of an opening up theme. It invites interpretation of science in terms not stated literally by the scientist—or, at least, not given emphasis by Galton—terms that refer to the scientist's conceptions of “what we can do.” In this way, my social interpretation of Galton's concern with regression invites delving further into the complexities of this and related cases even as the interpretation itself is easy to absorb (Taylor 2005). In summary, my account of Galton illustrates two more themes, which build on themes 2-5:

Theme 6:

In his understanding of regression and correlation, Galton's investigations of *biological* heredity incorporated a concern with justifying *social* inheritance.

Theme 7:

The theories of scientists can be interpreted as representing more than what they explicitly refer to.

III. Why we might be concerned about Galton's concern about “regression to the mean.”

Most statisticians who use the term regression do not know its original connotations, let alone share Galton's concerns. Nevertheless, certain social and scientific issues opened up by Galton's concerns about regression towards the mean remain relevant today. Let me identify three of these.

The first issue concerns the ideal of *meritocracy*, in which social resources are allocated on the basis of merit, somehow defined, and not according to family membership (i.e., social inheritance) or other forms of patronage. As defined, this ideal does not exclude biological inheritance of merit (or of inborn potential to gain merit). However, attempts to bring such inheritance into meritocracy can be influenced by assumptions about social possibilities. In the meritocratic vein, Galton, as we have seen, implied that the “ablest intellects” could be recognized—at least in the fields of science and literature. Yet, if each generation were able to identify its ablest intellects and promote them to high social position, then the quality of these leaders would not be diminished by regression towards the mean among the offspring of the previous generation's leaders. This was not a line of thought that Galton pursued. We might be surprised if he had given that identification of the ablest offspring across the whole of society and educating them accordingly was not something that he or his contemporaries could do (or imagine doing). Galton's vision of human betterment centered instead on ensuring eugenic marriages, which would be an extension of a practice that was very familiar and do-able among Galton's class, namely, the choice of marriage partners with a view to advancement of one's social and financial position. The benefit of such marriages for society's leaders and elites would be diminished by regression towards the mean for their offspring. Regression would slow progress towards a

future in which the merit of the elite—Galton's “galaxy of genius”—would be based in their biological inheritance.

It is interesting to probe the social commitments of those in more recent times who expound biological inheritance of merit. By the mid-twentieth century, compulsory schooling and intelligence testing seemed to make a meritocratic allocation of resources appear something we in liberal democracies can do. Consider, then, the case of H. J. Eysenck, a leading proponent in England of intelligence testing. Eysenck arranged for Galton's *Hereditary Genius* to be reprinted in 1978 and in his introduction referred to Plato's ideal of rulers being charged (by the gods) to

“scrutiniz[e] each child to see what metal has gone to his making, and then allocate or promote him accordingly” (Eysenck 1978, i). This introduction celebrates Galton as the founder of behavioral genetics, a field that has established the “strong genetic determinants of ability” (Eysenck 1978, i) and shown that the “whole pyramidal structure of... all advanced societies... is probably in large measure due to the inherited inequalities in mental ability” (Eysenck 1978, vi). Box 1 presents an exchange with Eysenck (an imaginary one; he died in 1997) to call forth what is implicit in this explicit advocacy of meritocracy based on biological inheritance.

Box 1:
An imaginary exchange with Eysenck about meritocracy

Taylor [T]: Why are you so concerned about genetic determination of mental ability?

Eysenck [E]: If abilities are determined by birth and society can predict who will be naturally talented, then it can allocate its resources more efficiently, for example, through separation of school children into separate tracks.

T: Why not test young people and use the results to make such predictions—then we can forget the issue of where their abilities originate? You have, after all, been a life-long proponent of mental testing.

E: If abilities are biologically inherited and society is meritocratic, then elites are biological elites.

T: And so...?

E: Rather than wait until children are old enough to be tested for intelligence, we can allocate resources from birth onward according to their parents' status.

T: High status parents already do that. Wouldn't someone who does not believe in meritocracy—someone who prefers a system that perpetuates privilege—also support the practice you propose?

E: The difference is that I would use intelligence tests at eleven, sixteen, and so on to check that the right children have been placed on the advanced tracks.

T: Then, again, why not simply use such testing and forget the heredity issues?—especially given that parental intelligence is an imperfect predictor of offspring intelligence.

E: Even if starting to track children at an early age leads to some errors, it is probably a more efficient allocation of educational resources.

T: Efficient for whom?—You must know that tracking in practice means more than providing different kinds of education; time and again it has resulted in unequal allocation of resources (Oakes 2005).

E: That does not have to be the case.

T: Maybe not, but unless you can show that unequal allocation has never been the case in the past, how could you show that the current “pyramidal structure” of society is due to “inherited inequalities in mental ability”?

The last question of the exchange with Eysenck points to a larger methodological issue: how can intelligence testers and behavioral geneticists tease apart the various contributions to the development of intelligence? The tests that Eysenck proposes above for adolescents must also reflect the effects of any differential allocation of educational resources since birth. Even if intelligence testers could discount these effects, what do they envisage policy-makers being able to with their claim that intelligence is genetically determined? To push researchers on these questions is to probe their social commitments, to attempt to expose their views on what we can or could do. As historian of science Carson (2006) describes, the source and significance of inequalities in people's talents have been revealingly contentious issues since the Enlightenment.

A second important issue opened up by Galton's concerns about regression is the *persistence of typological thinking*. This aspect of Galton's worldview is evident when we still say, for example, that "men tend to be taller than women," or "men, on average, are taller than women." (The emphasis here is on the type, with the deviation from it a secondary matter. Have you ever heard someone try to subvert this by saying "the spread of heights for men is centered at a higher value than the spread of heights for women"?) When the more careful terms of statistical analysis are used, measurements of the heights of men and women are compared by examining the differences between the means of men's heights and the means of women's heights in relation to the spread of values around these means (using the "t test"). Deviation is taken into account yet such analysis treats the spread away from the mean as if it happened by chance. In other words, the mean is the real value around which there are chance deviations. Now, it might be objected that a typological worldview does not apply to the analysis of correlations between sets of measurements; correlation, after all, focuses not

on the means but on the variation. (To back up this objection, notice that the slope of the regression line can be high or low independently of the relative sizes of the means of the correlated variables.) My response would be that typological thinking persists in this area as well, to the extent that statisticians portray this variation as a spectrum of types that any observed individual is an imperfect expression of. Let me give an example of typological thinking regarding correlation.

Stigler, a statistician as well as a historian of statistics, presents a way to visualize regression to the mean that revolves around dividing each observation into a "permanent component [due to skill and] a degree of luck (a transient component)" (Stigler 1997, 104). He invites us to imagine that we observe the same individuals twice, measuring the same trait, say, the score in two exams on the same subject. He argues that many who scored highly on the first will have done so because their luck component was positive. They would retain their skill component the second time but have luck that might be negative or positive. Among a group of such people, luck will average out to zero; in other words, their second scores will average lower than their first—hence, there will be regression.

Stigler's typological construal of correlation makes it hard to visualize that there may be a diversity of processes of development leading to the different data points. *Neither luck nor an unchanging type is required to produce the pattern of regression towards the mean.* To illustrate this counterpoint, consider the following, non-typological alternative to let me Stigler's verbal explanation of regression. Recall that the regression line is the "best predictor" line for a cloud of points that represent two measurements on each member of a set of individuals. (That is, this line can be used to predict one measurement based on the other. The prediction is not perfect, but the discrepancies are the minimum possible.) Now

let an ellipse stand in for that cloud (figure 6). If the measurements have, as before, been scaled so that the spread (standard deviation) is equal in both directions, the long axis of the ellipse will be a line of slope 1. Any vertical slice of that ellipse on the right will have fewer values above its long axis than below. (Readers can check this readily by drawing an ellipse and rotating it around its center.) The average of the values in any slice must be less than the value on the long axis; the best predictor line has to go as close as possible to the averages of all the slices; the best predictor or regression line must, therefore, have a slope less than 1. (For the actual cloud of data points, there may be some vertical slices of points on the right of the center that happen to have an average above the long axis of the ellipse, but as we move towards the slices on the far right, the ellipse is thinner and that occurrence will be rarer.) Once one sees that vertical slices of any ellipse sloped at an angle to the horizontal will have this property nothing more is needed to understand the pattern of regression towards the mean. In sum, although it remains an open question what the processes were that generated any specific data points, these processes need not be conceived in Stigler's typological way.

In light of this last point, a third issue opened up by Galton's concerns is how directly we expect *patterns to provide insight about processes*. Galton chose the term regression because he saw it as a dynamic process counterposed to another dynamic process, namely, "deviation" (from the mean). Even though he came to appreciate that regression towards the mean occurred in any set of correlated data, the connotations of process colored his interpretation of the patterns: "The selection of the most serviceable variations cannot even produce any great degree of... improvement, because an equilibrium between deviation and regression will soon be reached" (1892a, xviii; see Porter 1986, 287-9).

When statisticians today use the term regression and deviation, the connotations of

process that held for Galton are no longer obvious. (Indeed, "regression" is now used as a purely technical label for formulas that best predict one measurement on the basis of other measurements.) Nevertheless, a legacy remains in how the pattern-to-process relationship is conceived. Consider the concept of a regression line as a best predictor line. To predict one measurement from another is to hint at, or to invite, causal interpretation. Granted, if we have the additional information that the second measurement follows the first in time—as is the case for offspring and parental traits—a causal interpretation in the opposite direction is ruled out. But there is nothing about the association between correlated variables, whether temporally ordered or not, that requires it to be assessed in terms of how well the first *predicts* the second (let alone whether the predictions provide insight about the causal process). After all—although this is rarely made clear to statistics students—the correlation is not only the slope of the regression line when the two measurements are scaled to have equal spread, but it also measures how tightly the cloud of points is packed around the line of slope 1 (or slope -1 for a negative correlation). (Technically, when both measurements are scaled to have a standard deviation of 1, the average of the squared perpendicular distance from the points to the line of slope 1 or -1 is equal to 1 minus the absolute value of the correlation [Weldon 2000]. This means that the larger the correlation, the tighter the packing.) This tightness-of-packing view of correlation affords no priority to one measurement over the other. Whereas the typical emphasis in statistical analysis on prediction often fosters causal thinking, a non-directional view of correlation reminds us that additional knowledge always has to be brought in if the patterns in data are used to support causal claims or hypotheses.

This last point recalls the classroom activity in section I. With regard to the need for additional knowledge in order to support causal claims, it is no revelation that, even if these

heights of offspring and same-sex parent are correlated, the process of reproduction and development of height involves the other-sex parent as well! But there are less obvious or more contentious patterns in parent-offspring correlations that are not easy to interpret in terms of process. For example, correlations among genetic relatives in IQ test scores coexist with a generational trend in Western societies of increases in a population's average IQ test score (Flynn 1994). There has been no genetic change in the population to account for the trend; nor is there any simple environmental explanation, such as dietary improvement or increased years in school. To account for the pattern, we need to investigate the more complex dynamics of individual development and social change. We might need to take into account the ways in which a child can elicit responses from parents, responses moreover that may be modulated by the prevailing social customs (Dickens and Flynn 2001). We might also need to expose heterogeneous factors underlying the development of the "same" value of a trait among different individuals (Taylor 2008). Indeed, if researchers find a way to explain the processes of development of human traits in a way that encompasses both the patterns of average increase in IQ test scores and their variability, they should be able to offer insights about what we can do to promote human betterment—insights that eluded the typological-thinking, pattern-process-conflating Galton.

In summary, probing Galton's concern about "regression to the mean" allows us, in the spirit of themes 1 and 3, to open up social and scientific issues that still require examination and clarification to this day:

Theme 8:

Discourse about meritocracy can be confused or confusing about whether the central concern is allocation of social resources based on merit or justification of social inheritance.

Theme 9:

Typological thought persists in statistical analysis and explanation.

Theme 10:

Drawing direct connections from pattern to process distracts us from paying attention to more complex dynamics of development of human traits in a social context.

Conclusion

In the Introduction I raised the very broad question of how interpretation can guide people in effecting change. In the context of interpreting *scientific activity*, I noted a tension between accounts of the particular complexity of the diversity of things scientists do in practice and simpler themes that are more readily conveyed and digested by a wider audience. Reflecting on what I, as a teacher and writer, can do, I chose for this essay not to delve into the idiosyncratic complexity of some specific case of science-in-process. Instead, I set out to motivate simple themes of a certain kind, namely, those that open up questions and point to further work needed to grapple with the complexities in particular cases. Galton's work that made foundational contributions to the development of statistics cannot be explained simply in terms of an ideology supporting the perpetuation of privilege. Yet, by examining the logic of his inquiries (and his confusions) regarding biological inheritance, I was able to point to his implicit concern with justifying *social inheritance*. Further historical research on this concern is called for and I point to certain social and scientific issues that still require examination and clarification, namely, confusions about the relationship between meritocracy and heredity, the persistence of typological thinking, and how directly we expect patterns to provide insight about processes.

Will the expository-pedagogical approach employed in this essay achieve its aim of stimulating readers to think about aspects of

complexity they had not given much or any attention to before? I cannot be sure, but I look forward to hearing reader's responses, learning from them, and thus widening the discussion of how interpretation of and in science is meant to guide people in effecting change.

Coda

If the preceding, tentative conclusion seems to leave too much up in the air, I can point to something definite: The simple, opening-up themes introduced in this essay have been informing and guiding my own inquiries. In recent years I have focused on methods for unraveling the complexities of developmental processes that vary among the array of individuals (Taylor 2004). I have been especially interested in social epidemiologists' worries about the causal import of their analyses of observations (e.g., Davey Smith and Ebrahim 2007) and the tension between making population-based public health policy and promoting research into diverse individual etiologies (Rose 1985 [2001]). Discussions in these areas have led me to keep probing the typological thinking that persists in statistical methods. As a coda, let me briefly explain why. Typological thinking is not necessarily illegitimate. We can readily imagine, for example, a comparison of the dental health of two communities that are similar except that the one with better average dental health has naturally high level of fluorides in its water supply. If the variation around the averages is small relative to the differences in the two averages, it seems reasonable to prescribe fluoridisation of water supplies lacking natural fluoride. In doing so—or supporting those who do so—we discount the variation around the average and other deviations from type, such as teeth discoloration that occurs in some individuals. We discount the variation because the benefits exceed the costs when summed up for the community (or population). Public health policy-makers are able to do this (recall theme 2) as long as the individuals who bear

disproportionate cost do not effectively mobilize resources and allies to resist.

But suppose now that two "racial" groups—race defined, say, as in census categories—show persistent differences on average in scholastic achievement tests. By analogy with the fluoride case, we should ascribe the difference to race, that is, to some social or biological variable or variables that differ from one race to the other. Identifying those variables won't be as simple as noting the presence or absence of fluoride, but should we try to find them? What if we were to succeed?—If the variable were unchangeable (say, a matter of genes), would we resign ourselves to the difference? If the variables were biologically or socially changeable, would we administer an "antidote" to all in the lower-achieving group?

We might be inclined to say yes to this last question, just as we advocate fluoridisation. Yet there may be many subsets of one racial group who share more biological and social factors influencing the development of their scholastic achievement test score with subsets of the other group than they do with other members of their own group. The best way to improve the average test score of the group with a lower average may be to explore the particular combinations of factors underlying the development of test scores for the different subsets, not to look for and act upon the two groups as a distinct wholes. Indeed, racial stereotyping builds on the assumption that factors shared within racial groups predominate over the particular combinations of factors for subsets of individuals within and across groups (Taylor 2008). Ironically, by contributing to the routine treatment of individuals in U.S. society according to their racial group membership, the stereotyping assumption may well have generated some important factors that *are* shared within each group and not across.

The relationship of interpretation to change in this second case of average test score differences involves even more complexity. If, say, we try to shift the focus from group

membership to reconstructing diverse pathways of development, we risk bolstering the fiction that racial group membership no longer brings social costs or benefits—or, at least, that racial categorisation should not be used in policy as an indicator of whose development has been hindered or enhanced by those costs or benefits. There is a lot more to tease out in cases like this, but not here. Let me close this essay by proposing that new methods that counteract persistent typological thinking are needed if we are to interpret such social and scientific complexity in ways that help us change it.

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RESUMEN

Este ensayo interpreta las preocupaciones explícitas de Francis Galton con respecto a la herencia biológica en términos de sus opiniones implícitas sobre el rol que debe desempeñar su estrato social. El patrón de regresión implica que los individuos excepcionales en una generación no pueden confiar en la herencia biológica para garantizar que sus descendientes sean parte de los individuos excepcionales de la generación siguiente. La interpretación de las preocupaciones de Galton sobre regresión apunta a asuntos sociales y científicos que todavía requieren de análisis y clarificación: confusiones sobre la relación entre la meritocracia y la herencia; la persistencia del pensamiento tipológico; y la existencia de expectativas claras de que los patrones estadísticos proporcionen elementos de análisis para la interpretación de ciertos procesos. Una actividad educativa, diseñada para un salón de clases, se presenta para establecer el tono pedagógico de este ensayo, prestando énfasis a conceptos fundamentales accesibles

a quienes no son especialistas en estadística y presentando una serie de temas que abren la investigación hacia otras complejidades. El ensayo en conjunto ejemplifica y promueve una atención más reflexiva a las maneras en que nuestras interpretaciones científicas y sobre la ciencia intentan promover el cambio en la ciencia y en la sociedad.

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