

ROUNDTABLE COMMENTARIES

Fraternal Birth Order, Maternal Immune Reactions, and Homosexuality in Men

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In my comments on Townsend's detailed critique of Sulloway's (1996) book, I want to make two general points about birth order research. The first is that several authors—including Ernst and Angst (1983), who are extensively quoted by Townsend—have concluded that the effects of birth order on adult personality and behavior are either completely nonexistent or else so negligible as to be useless to science. I agree that the methodology of birth order studies is often flawed, and that many, if not most, of their findings are probably irreproducible. However, an assertion that birth order has *no* effect on adult behavior would be as extreme in its way as the assertion that birth order's effect on behavior is decisive. My own research demonstrates that a categorical dismissal of any and all birth or-

der effects is not only premature but demonstrably erroneous.

My colleagues and I at the Clarke Institute of Psychiatry in Toronto (now called the Clarke Division of the Centre for Addiction and Mental Health) have been investigating the relation between birth order and sexual orientation for over 10 years. Our studies to date (reviewed in Blanchard, 1997) have repeatedly shown that homosexual men have a higher mean birth order (i.e., they have more older siblings) than do comparable heterosexuals. This difference has now been demonstrated in subjects examined in recent years and subjects examined decades ago; in groups collected in England, The Netherlands, Canada, and the United States; in psychiatric patients and in nonpatient volunteers; in subjects examined in adulthood and subjects examined in childhood; in men who wish they were women and in men contented with their male role and anatomy; and in men sexually attracted to adults as well as men attracted to children. The collective evidence, therefore, suggests that a high birth order is associated with homosexuality in men, regardless of their other cultural, demographic, or psychological characteristics.

We have further shown that homosexual men have a higher birth order than heterosexual men, primarily because they have a greater number of older brothers. They do not differ with regard to older sisters, once their number of older brothers has been taken into account (Blanchard and Bogaert, 1996a, 1996b, 1998; Blanchard et al., 1998; Blanchard et al., 2000; Bogaert, Bezeau, Kuban, and Blanchard, 1997; Ellis and Blanchard, 2001; Green, 2000; Jones and Blanchard, 1998; see also Blanchard and Bogaert,

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**Editor's Note:* Since the article in this roundtable is a focused critique of a book—Frank J. Sulloway's *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives* (1996)—we made some adjustments in our normal roundtable format. Thus, to ensure representation of viewpoints sympathetic to Dr. Sulloway's analysis, we invited Dr. Sulloway to suggest the names of potential commentators. Half of those invited to write commentaries (and half of the commentaries published here) were suggested by Dr. Sulloway. As always, the author of the roundtable article—in this case, Mr. Townsend—has contributed a response. Under the adjusted format, however, Dr. Sulloway has also contributed a response. Dr. Sulloway was permitted 20,000 words for his response, a figure equal to the combined word count of Mr. Townsend's article and the permitted length of Mr. Townsend's response. Dr. Sulloway is responding *both* to Mr. Townsend's article and to all of the commentaries, but not to Mr. Townsend's response, which he has not yet seen. Mr. Townsend is responding only to the commentaries; he has not yet seen Dr. Sulloway's response.

1997; Purcell, Blanchard, and Zucker, 2000; Williams et al., 2000; Zucker et al., 1997). In other words, the probability a man will be homosexual increases only in proportion to his number of older brothers; older sisters neither increase nor decrease the probability of homosexuality in later-born males. This phenomenon has therefore been termed the *fraternal birth order effect*.

In contrast to our data on males, our data on females, no matter how they are analyzed, reveal no consistent tendency toward early or late births in homosexual women (Blanchard, 1997). It thus appears that the relation between birth order and sexual orientation pertains only to males. Females do not influence their siblings' sexual orientation, and their siblings do not influence theirs.

My second general point is that birth order effects may proceed from prenatal, biological events rather than postnatal, experiential ones. In fact, I and my colleagues have argued that maternal immune reactions are the most plausible explanation for the epidemiological data reviewed above.

Because of the finding that females are essentially invisible to the fraternal birth order phenomenon, Blanchard and Bogaert (1996b) conjectured that male homosexuality may result from a maternal immune reaction, which is provoked only by male fetuses, and which becomes stronger after each pregnancy with a male fetus. This hypothesis was based partly on the argument that a woman's immune system would appear the biological system most capable of "remembering" the number of male (but not female) fetuses that she has previously carried and of progressively altering its response to the next fetus according to the current tally of preceding males. It should be stressed that Blanchard and Bogaert (1996b) did not hypothesize that maternal immune reactions are the only, or the most important, cause of homosexuality in men.

A maternal immune hypothesis of homosexuality had previously been proposed by MacCulloch and Waddington (1981), who suggested that the relevant fetal antigen is testosterone. They speculated that antibodies to testosterone, produced by a woman pregnant with a male fetus and passed through the placenta from the mother to the fetus, could reduce the hormone's biological activity and thus compromise the sexual differentiation of the fetal brain.

MacCulloch and Waddington's (1981) candidate for the immunizing agent is unlikely, because steroid hormones are not ordinarily antigenic. Therefore, Blanchard and Bogaert (1996b) theorized that the relevant fetal antigen might be one of the male-specific, Y-linked, minor histocompatibility antigens, often referred to collectively as H-Y antigen (for reviews, see Müller, 1996; Wolf, 1998). H-Y antigen almost certainly has some role or roles in the sexual differentiation of vertebrates (Wachtel, 1983), because it is usually present in the heterogametic and absent in the homogametic sex—in mammals, present in males and absent in females—and because it has been highly conserved

throughout vertebrate evolution (Nakamura et al., 1987; Wachtel, Koo, and Boyse, 1975).

Various lines of indirect evidence supporting the hypothesis that maternal antibodies to H-Y might influence sexual orientation have been summarized by Blanchard and Klassen (1997). Such evidence includes animal research establishing that the maternal immune system does recognize and react to fetal H-Y antigen; epidemiological data suggesting that male fetuses are more antigenic to human mothers than are female fetuses and more likely to provoke maternal immune reactions; studies of tissue localization indicating that H-Y antigen is strongly represented on the surfaces of brain cells; and research on mice suggesting it is plausible that H-Y antibodies could be present in sufficient quantities to affect sexual differentiation in the fetal brain, without also affecting the development of the genitalia. Of particular interest is the finding of the single most relevant animal study: Singh and Verma (1987) reported that male mice whose mothers had been immunized to H-Y prior to pregnancy were much less likely to mate successfully with receptive females. None of the aforementioned lines of evidence constitutes proof that maternal immune reactions underlie the fraternal birth order effect, but they do demonstrate that an immunological explanation is at least as promising as the various, in practice untestable, psychosocial hypotheses that have been advanced (see Blanchard, 1997).

In summary, I have drawn on my own work in this commentary for concrete illustrations of two points related to Townsend's article. The first is that every birth order finding and every birth order theory must be evaluated on its own merits—as Townsend has done in evaluating Sulloway's work. Negative reviews of the published birth order literature may be perfectly fair evaluations of the studies completed to a given date, but their conclusions cannot be extrapolated to mean that future birth order research will never produce anything of scientific interest.

My second point is that researchers should be more aware of the possibility that biological factors might mediate correlations of birth order with behavioral or characterological traits. There is some evidence, although much less than for homosexuality, that fraternal birth order also correlates with mental retardation in males (Ackerman, Goolsby, and Paal, 1988; Flannery and Liederman, 1994; Lord, 1992). Such a finding, if reliable, would certainly suggest a biological cause, and in particular, an immunological one. There is no reason to dismiss the possibility that biological factors correlated with maternal parity (or gravidity) could also produce benign variations in human behavior.

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In Expectation of Meta-Analysis

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I am surprised and glad that our book, *Birth Order* (Ernst and Angst), which appeared in 1983, is still quoted in a controversial discussion. We wrote the book with little support and even less sophistication. By the vote-counting method, the result was unequivocal: birth order did not appear as an environmental factor with predictable effects on personality or psychiatric disorder. When the book was written, we were unable to use meta-analysis. If someone found it worthwhile and possible to reanalyze by meta-analysis the studies mentioned in our book or published later, the result of this effort could determine the value of doing any further research.

Since 1983, the change in developmental psychology that impressed me most was the genetic research done by

Plomin and Kendell and their schools. Their twin and adoption studies led to the conclusion that about half of the variance in personality traits is genetic. The environmental variance is not due to the environment children living in the same family share with each other. On the strength of their individual personalities—of their vulnerability and resistance—children shape their own parents, peers, and teachers, and thus produce themselves the interactions with the environment they are shaped by.

The hypothesis that birth order has predictable effects on personality conforms to this genetic theory in that the environment is supposed to be different for each member of a sibship. There is, however, a second assumption: that there is *a shared environment between children living in different families* in so far as they are of the same birth order. This similarity between families is believed to consist of similar attitudes of parents and children as a function of a child being first- or laterborn. Shared interfamilial influences would have to be very strong and very uniform between families to be able to influence personality while

a shared intrafamilial environment does not, or appears quite weak. Actually, neither the shared intrafamilial nor a supposed shared interfamilial environment seems to be important for personality development.

Birth order as an environmental variable has been compared to a vampire, which cannot be laid to rest. When we started to collect research data I remembered the Grimms' fairy tales: the stingy, diffident, and treacherous oldest, who does not marry the princess, and the happy, curious, and courageous youngest, who does. Probably in every culture birth order has been a source of myths and unforgettable images. Very likely we will have to put it to rest as an influence on personality, but with regret and a dignified burial.

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Personality and Birth Order: Explaining the Differences Between Siblings

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What struck me most forcefully in Townsend's critique (this issue) was a pattern he observed: A large preponderance of the changes he documented in Sulloway's numeric or graphic descriptions of data resulted in improvements. The later data point was almost always a better fit to Sulloway's predictions than the earlier one. Townsend suggested that further analysis

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would be appropriate, which made me wonder why he stopped where he did, without doing any statistical tests on his findings. A possible next step would be to calculate the likelihood that the observed proportion of changes in the confirming direction occurred by chance alone.

Townsend also mentioned the confusion caused by the muddling of the distinction between "studies" and "findings." Sulloway (1995, 1996, 1998a) repeatedly referred to the 196 controlled "studies" he had found in Ernst and Angst (1983); a footnote under a table (Sulloway, 1995:78n; 1996:73n) explained that "study" actually meant "finding." The distinction is important because a single study, involving a single sample of subjects, can give rise to multiple findings. I have been incorrectly accused (Sulloway, 1998b) of ignoring that footnote and counting studies instead of findings in Ernst and Angst,¹ but it was Sulloway himself who mixed them up—for example, by claiming (1995:77; 1996:72) that the "196 studies" involved "120,800 subjects" (there were 120,800 subjects only if some subjects were counted multiple times), and that the results of his analysis passed Rosenthal's "file-drawer test." Sulloway (1995:79)

used Rosenthal's (1987:224-25) formulas to reject the possibility that there were enough nonsignificant studies sitting around in file drawers to invalidate the results of his analysis. But where Rosenthal's formulas called for K , the number of published studies, Sulloway had inserted 196, the number of published *findings*, overlooking Rosenthal's stipulation that these formulas are based on the assumption "that each of the K studies is independent of all other $K - 1$ studies, at least in the sense of employing different sampling units" (Rosenthal, 1987:224).²

Townsend's discoveries are a reminder that, in judging the merits of a theory, it is best not to rely on evidence provided by the originator of the theory. As McDonagh pointed out, "The dialogue that is the very process of science insists that studies be replicated by researchers independent of the original researchers before the scientific community at large accepts the findings as valid" (2000:678). Thus, the real test of Sulloway's theory is not whether Sulloway himself can find evidence to support it: The question is whether it is supported by evidence gathered and analyzed by other investigators. I will describe new studies, carried out since the publication of *Born to Rebel*, that provide clear tests of Sulloway's theory. The results do not support the theory.

Why Are Siblings So Different?

Sulloway's theory is an attempt to use concepts from evolutionary psychology to solve a puzzle turned up by behavioral geneticists. (Full disclosure: I have proposed a competing theory, also based on concepts from evolutionary psychology, in an attempt to solve the same puzzle.) Behavioral genetic studies of adult personality have consistently shown that about half the variance in the measured characteristics can be attributed to genetic differences among the participants. The other half is environmental, but its source is mysterious: Little or none of it can be attributed to the home environment shared by siblings who grew up together (see Harris, 1995, 1998; Rowe, 1994). Thus, roughly half the variance in personality is unexplained by traditional theories of "nature and nurture."

There are sizable nongenetic differences between siblings that cannot be blamed on any environmental influences they had in common while growing up. Sulloway's theory and mine provide alternative explanations of these differences.

Of course, other investigators are also trying to explain the differences between siblings. There are two recent reports from researchers who made extensive efforts to find the source of these differences and who had (at least at the outset) no theory of their own to promote. The first, by Turkheimer and Waldron, is a meta-analysis—a real meta-analysis, not a vote-counting procedure.³ Turkheimer and Waldron reported that effect sizes were "smallest for stud-

ies examining family constellation variables such as differences in birth order and age spacing" (2000:89). Effect sizes for these four studies were small indeed: The weighted mean r^2 (variance accounted for) was .01.

Reiss (2000) reported the results of a single study—a large and exceptionally sophisticated one. He and his colleagues studied 720 adolescent sibling pairs (twins, ordinary siblings, half-siblings, and stepsiblings) over a three-year period. The siblings' behavior and adjustment, their relationship with each other, and the parents' behavior toward each of them were judged by observers, the parents, and the siblings themselves. Reiss concluded that neither "differential parenting toward siblings" (e.g., the favoring of one child over another) nor "asymmetrical relationships the sibs construct with each other" (e.g., dominance of the younger by the older) could account for the measured differences between siblings (2000:407).

Turkheimer and Waldron (2000) and Reiss (2000) came to the same conclusion: that so far researchers have been unable to identify the environmental factors that make siblings—even reared-together identical twins—different from one another. Their findings showed that birth order was not the answer.⁴

Different Methods, Different Results

Several investigators have tested predictions generated by Sulloway's theory. Most (e.g., Beer and Horn, 2000; Freese, Powell, and Steelman, 1999; Hauser, Kuo, and Cartmill, 1997; Jefferson, Herbst, and McCrae, 1998) have concluded that their results failed to support the theory. For example, Freese, Powell, and Steelman used questionnaire data from 1,945 adult respondents to test Sulloway's (1996) claim that firstborns are more conservative, supportive of authority, and "tough-minded" than later-borns. "We find no support for these claims," the researchers concluded, "either in terms of significant effects or even the direction of nonsignificant coefficients" (1999:207).

However, studies of one particular kind did support Sulloway's predictions. These were studies (e.g., Paulhus, Trapnell, and Chen, 1999; Salmon and Daly, 1998) in which subjects were queried about their feelings regarding their parents or siblings, or asked to compare themselves with their siblings or their siblings with each other. Paulhus, Trapnell, and Chen, for example, found significant birth order effects when they asked 1,022 adults to indicate which member of their family was the most rebellious and which was the academic achiever. Laterborns were most often named (or named themselves) as rebels; firstborns were identified as academic achievers.

The trouble with these all-in-the-family studies is that their results do not correspond to objective measures made in the world outside the family. People may believe that their youngest sibling is the family rebel and that the oldest

is the academic achiever, but large quantities of data indicate that laterborns are not more likely to rebel against authority by underachieving in school or dropping out; nor are firstborns more likely to graduate from high school and go to college (Blake, 1989; McCall, 1992).

Studies using standard self-report tests of personality or social attitudes generally fail to find significant birth order effects or find negligible effects that do not hold up from one study to another. Sulloway has offered two explanations for these negative outcomes. First, studies that do not compare siblings in the same family might produce spurious results due to “confounding effects associated with differences between families” (1999:192). However, some of the researchers who used standard self-report tests (e.g., Freese, Powell, and Steelman, 1999; Hauser, Kuo, and Cartmill, 1997) performed within-family analyses, directly comparing the responses of siblings in the same family, and nonetheless failed to find birth order effects.⁵

Sulloway’s second explanation (1996, 1998b, 1999) for the negative results obtained with standard personality tests is that the tests are invalid, either because subjects do not answer them honestly (“How many firstborns are willing to describe themselves as ‘callous’ or ‘unadventurous’?” he asked in *Born to Rebel*, p. 474) or because the measures are “unanchored” (1998b). As Jefferson, Herbst, and McCrae observed, however, “There is vastly more evidence supporting the validity of self-reports than there is supporting effects of birth order” (1998:507). More to the point, the sibling differences Sulloway is trying to explain were discovered in studies using the very method he is criticizing. His theory was designed to account for the unexplained variance in personality, and what that means, for the most part, is variance in scores on standard self-report personality tests.

Reconciling the Discrepant Results

Sulloway is right about sibling rivalry: Siblings do compete for family resources and parental attention. Birth order affects the way people behave toward, and feel about, their parents and siblings; these behaviors and feelings often last a lifetime. But, as Ernst and Angst hypothesized in 1983 (p. 171), these behaviors and feelings are specific to the family environment. The evidence (see Harris, 1998, 2000a, 2000b) indicates that the patterns of behavior children acquire while learning to get along with their siblings are not transferred to outside-the-home contexts and relationships. For example, children who are dominated by older siblings at home are no more likely than firstborns to allow themselves to be dominated by their peers (Abramovitch, et al., 1986).

The context-specificity of birth order effects is clearly demonstrated by the results of another recent study, by Deater-Deckard and Plomin (1999). When judgments are

made by parents or siblings, firstborn children are sometimes described as more aggressive than laterborns (Ernst and Angst, 1983)—a result in accord with Sulloway’s predictions. But what if you ask their teachers? Deater-Deckard and Plomin asked both parents and teachers to judge the aggressiveness of the older and the younger of a sibling pair (156 adoptive siblings and 188 biological siblings, assessed five times between the ages of 7 and 12). There was no main effect of birth order on aggressiveness, but there was a significant interaction between birth order and source of information: Parents judged the older child to be more aggressive than the younger one; teachers did not. Apparently the older one was more aggressive at home but not at school.

There is a reason why so many people (and so many psychologists) believe that birth order is important: They can see that it influences behavior and relationships within the family, so they assume that it must affect behavior and relationships outside the family as well (Harris, 1998, 2000a, 2000b). But this assumption is incorrect. Even in childhood, birth order effects are not carried along to other social contexts—and why should they be? A child who learns at home that he can push around his younger sibling might turn out to be the smallest and weakest in his kindergarten class; it would not make sense for this child to behave with his peers the same way he behaves with his sibling.

Evolution has designed young humans wisely. Birth order does not affect adult personality because children are not destined to spend their adult lives in the bosom of their family of origin.

Notes

1. In fact, I counted both studies and findings in Ernst and Angst’s (1983) survey and reported both results: 179 findings, 116 studies (Harris, 1998:368-69). My tally of their findings, and further discussion of topics touched on in this commentary, are posted on the birth order page of *The Nurture Assumption* website (<http://xchar.home.att.net/tna/birth-order/>).
2. Rosenthal (1987:224-25) gave a formula for estimating the number of unpublished studies sitting around in file drawers ($5K + 10$, where K is the number of published studies) and a formula for calculating how many unpublished studies it would take to invalidate the results of a summary of published studies ($19s - n$, where s is the number of significant published studies and n is the number of nonsignificant published studies). Sulloway (1995) evidently made $K = 196$, $s = 72$, and $n = 124$; his results were 990 and 1,244. He concluded, “This number (1,244) exceeds 990, indicating that the published findings pass the file-drawer test” (1995:79).
3. The term *meta-analysis* is normally reserved for a procedure that takes into account effect size and the number of subjects in each of the included studies.
4. Indeed, it is hard to see how birth order could account for personality differences between twins. A more serious problem for Sulloway’s theory is that it is based on the premise that siblings have competing interests because they share only 50 percent of their genes (1996:60). Identical twins, who share 100 percent of their genes, show nongenetic differences in personality that are roughly equal in magnitude to the

nongenetic differences between nontwin siblings (see Harris, 1998).

5. The technique of making within-family comparisons among siblings has recently been used to look for birth-order effects on IQ (Rodgers, et al., 2000). No birth order effects were found. These results call into question the widespread belief that firstborns are more intelligent than laterborns.

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Birth Order and Rebelliousness: Responding to Birth Order Research Contradictions

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Although I am intrigued by the recent efforts of several researchers who have taken issue with Sulloway's (1996) work on birth order, I experience some hesitancy to comment for a number of reasons. Birth order research has historically come up with contradictory findings (Nyman, 1995). In the main, I suspect that the apparent inconsistencies will be resolved not through commentary but with keener measuring tools, better ex-

perimental controls, and with the patience and energy to define each research condition so as to enable accurate replication. It is also not an easy assignment to critique the work of scientists whom I admire, especially when their inferences seem to be at odds. With this said, I shall attempt to deal with design issues inherent in birth order research in an effort to illustrate some of the factors that enter into the contradictory findings reported in the literature.

It has been my impression from reviewing birth order research that even something as basic as the proper identification of the birth order position has posed a serious classification problem. This is particularly a problem in reconstituted families, in which the merger of step and half siblings brings about perceived and actual changes in birth order position. Individuals in such families often identify with more than one birth position. In these instances, the criteria for establishing the so-called functional birth position are unclear. Another example of problematic birth order designation is observed in *Born to Rebel* when Sulloway grouped only children with firstborns (1996:442). It is just as reasonable for only children to be categorized as lastborns. Age spacing also causes classification fudging. Toman suggests that "siblings who are six or more years apart tend to become quasi only children, unless one or both of them happen to be surrounded by other children that are closer in age" (1993:32). In Toman's schema, a family with three children separated by six or more years between siblings would be "composed" of three only children. In another example, Toman describes a family in which the first two siblings are separated by two years and the third child is separated from the second sibling by six plus years as a constellation of an eldest, youngest, and only child.

Such classification variations have been largely ignored in birth position research, and may have led to contaminated, perhaps meaningless findings by counting apples as oranges and pears as grapefruit. There is clearly a need to spell out birth rank measurement criteria. This viewpoint contrasts sharply with the opinion attributed to Ernst and Angst in Harris's paper on Sulloway's (1998b) interview with John Brockman: "Birth order research seems very simple, since position in a sibship and sibship size are easily defined" (Harris, 1998:6).

Sulloway (1998a) has responded in considerable detail to Harris's criticisms of his work. Different theoretical orientations between Sulloway and Harris lead to different interpretative leaps about the meaning of certain data. However, disagreement was also noted in respect to methodological tools employed in the gathering and examination of statistical data. Sulloway detailed a defense of his procedures. He supplied statistical rationales that justified his data analyses and pointed out errors in Harris's data tabulation. I am impressed with Sulloway's statistical sophistication; however, I find it difficult to adequately assess the assumptions that lead to the choice of one technique over another. I suspect that some of the contradictions found in birth order research

may mirror the appropriate or inappropriate selection of a specific statistical method.

Additionally, Sulloway pointed out that Harris's conclusions were flawed by her reliance on Modell (1997), who found that he was not able to replicate a vital section of Sulloway's work. Sulloway (1998a:4) reports on a correspondence in which Modell acknowledges an oversight in his reworking of Sulloway's data. This last point is of considerable importance in the present context, because Townsend (this issue) also relied on Modell's work to reject birth order effects.

Townsend, in his extensive summary of recent birth order research, refutes Sulloway's birth order theory and utilizes the Ernst and Angst (1983) data employed by Sulloway to punch holes in Sulloway's findings and methodology. Townsend's analyses of the Ernst and Angst data appear to contradict Sulloway's conclusions. Townsend argues persuasively when he combines controls for sibship size and social class as he compares the data found in *Born to Rebel*. Sulloway's response should help clarify the evident dissonance.

Townsend has also raised questions about the adequacy of Sulloway's data, especially in regard to historical review and methodology. He suggests that Sulloway has been selective in reviewing data, omitting and manipulating evidence to support birth order theory. These charges are made repeatedly and require examination. The thrust of Townsend's attack recognizes obstacles involved in birth order research, but at the same time minimizes the significance of Sulloway's efforts to deal with these issues, particularly in regard to classification and experimental controls. Townsend's views about inconsistencies and contradictions fail to fully appreciate or explore alternate explanations in the understanding of apparent differences in research findings. Townsend's critique is valuable in raising questions about methodology and experimental design; however, in what reads like the rejection of an entire body of work, some of Townsend's conclusions seem overstated and premature. Dissonance is an inherent part of many human conditions, and may be a measure of psychological complexity as well as a basis for theoretical and methodological modification.

In contemporary American society, perhaps in much of the Western world, there is an egalitarian view of family structure. This outlook is derived from the democratic ideology of equal and just treatment for all. This attitude is very often translated in the family to mean that all children in a family should be treated the same. There should be no favorites and no favored birth positions. Many parents go out of their way to reinforce this attitude, even when their hearts and behavior suggest otherwise.

In an attempt to determine whether people buy into an egalitarian family viewpoint, I asked college students to describe the essential characteristics of each birth position and rate these qualities in terms of their positive-negative attributes (Nyman, 1995). Regardless of the respondent's own

birth position, the results showed a clear perception that the eldest child held the most preferred birth position, followed by the middle siblings, the youngest, and only children at the low end. Clearly, young adults perceive significant differences in birth order positions.

Firstborns were described predominantly in positive terms. Firstborns were seldom subject to negative perception. In contrast, the middle, youngest, and only birth positions were described in terms that balanced positive and negative traits. Is the willingness to speak chiefly in positive terms of the firstborn position an idealized view of family structure? Or is it an authentic reflection of our preferences?

Data suggest that some birth positions and personality traits are viewed paradoxically. For example, youngest females were described as maternal/nurturant and immature/childish. There is also evidence that some personality characteristics are broadly shared across a spectrum of birth order positions, while other traits are associated with a single or a second birth position. For example, in the referenced study, caring and thoughtfulness were linked with all birth positions; leadership was identified only with firstborns; neglect, being overlooked, and confusion with middleborns; rebelliousness with middle and youngest children. The latter finding is supportive of Sulloway's hypothesis about laterborns.

Sulloway deserves enormous credit for the development of a unifying theory about birth position with testable hy-

potheses and procedures. He has single-handedly led the way in reawakening experimental interest in family structure. That alone is no small achievement. Townsend and others raise sound questions about data presentation and methodology. It is my opinion that fine-tuning in data gathering and measurement tools will augment the support for birth order effects. The best is yet to come.

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Birth Order and Revolutionary Leadership

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Finding ourselves in general accord with the thrust of Frederic Townsend's meticulous and unsparring critique (this issue) of Frank Sulloway's *Born to Rebel* (1996), we devote this commentary to an amplification of some of Townsend's points.

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We note at the outset that Sulloway would have been on firmer grounds had he limited his focus to prominent leaders of major revolutionary movements. "Rebel" and "rebellion" are exceedingly slippery terms, apply to a vast array of persons, circumstances, and events, and are virtually impossible empirically to define and measure. By contrast, "revolution" refers to a violent mass movement overthrowing a political regime as a step toward overall societal change; and prominent revolutionary leaders are those who play active and continuing roles throughout the process (see Rejai, 1977:chap. 1). (*Born to Innovate* would have been a more appropriate title for Sulloway.)

Over the last three decades, we have studied revolutionary leaders, loyalist leaders, military leaders, and American presidents—from Washington to Bush (Rejai and Phillips, 1977, 1979, 1983, 1988, 1996, 1997; Rejai,

Phillips, and Mason, 1993). Birth order was one of the many topics that occupied us throughout our studies.

Having focused on the revolutionaries first, and having drawn heavily on the literature of child development (see, for example, Broh, 1979, 1982; Forer and Still, 1976; Hermann, 1977; Mussen, Conger, and Kagan, 1969; Sutton-Smith and Rosenberg, 1970), we developed the following generalizations about oldest (or only) sons, youngest children, and middle children (see Rejai and Phillips, 1983:117-25; Rejai and Phillips, 1988:45-49).

Oldest children and oldest sons are typically held to strict and rigorous standards of performance, competence, and achievement. Inordinately high parental expectations generate acute anxiety, which requires reassurances that are (usually) freely given, which in turn set the stage for success. The firstborns' achievement orientation, in other words, is likely to lead to eminence in their chosen endeavors, revolution included.

Typically viewed and treated as "princes," oldest or only sons gradually respond to parental and social expectations by developing an aura of power, command, authority. At the same time, since the firstborn are more idealistic and likely to have strong consciences, they are more sensitive to acts of oppression, exploitation, brutality, or injustice when these occur. Moreover, given the fact that parents are likely to be more attentive and patient toward the firstborn, oldest children are likely to experience an orderly and coherent world in their early years. Accordingly, once they are adults and on their own, revolution may become a way of introducing order in a chaotic universe.

Oldest children are also likely to experience intense feelings of anxiety over the loss of exclusive parental affection and attention once siblings begin to arrive. Coupled with this anxiety are intense feelings of guilt over the hostilities the firstborn exhibit toward their siblings. Externalizing and politicizing these feelings of guilt and hostility—even to the point of revolutionary action—may be a way of managing one's psychic balance.

The family experiences of *only* children are closer to those of the firstborn than to any other sibling rank. They differ from the oldest children, however, in that they occupy the center stage from beginning to end, while at the same time being free from the anxiety and insecurity that the presence of siblings generates. Spending inordinate amounts of time with their parents, only children mature faster and are generally well-informed and intelligent. Moreover, they are found to be independent personalities and to make strong leaders who can have difficulty following orders.

Youngest children are more striving and more defiant toward their siblings—and probably toward the world in general. They are more competitive and more vigilant in an effort to maintain their status and possessions in the cruel world that a large family may represent. They may resort to devious means in order to achieve their objectives. Bold and aggressive, they are typically seen as the "conquerors."

Youngest children's sense of relative parental neglect and deprivation may generate repeated impulses toward rebellion. Moreover, being group oriented, youngest children may see revolutionary movements as a means of maintaining and enhancing their sense of identity and belongingness.

Middle children are underrepresented among revolutionaries because, as a group, they are not subjected to the same strict parental code and norms of behavior that are applied to the firstborn. Middle children are relatively neglected, and parental expectations are not as high. Being in relatively dependent and vulnerable positions, they tend to be pragmatic and conforming. They are likely to be "diplomats"—either as a means of working their way around older siblings or as a means of acting as mediators between older and younger siblings. On the whole, middle children are better adjusted and more content with their lives than either the firstborn or the last-born. Understandably, as a rule they do not seek radical change. There are times, however, when they may revolt against the dominance of the firstborn or imitate the rebelliousness of the firstborn and the last-born.

While these generalizations held up rather well in connection with the loyalists and revolutionaries, they completely broke down when applied to military leaders and American presidents, leading us to the conclusion that birth order is not a particularly important component of leader personality.

Parentetically—and for the record—we note that the 42 loyalists and 43 revolutionaries for whom we had sibling data came from large families, with an average of seven children in each household. Of the loyalist group, 31% (13) were middle children; 49% (19) were oldest children or oldest sons; 21% (9) were youngest children; and 2% (1) were only sons. As for the revolutionaries, 28% (12) were middle children; 37% (16) were oldest children or oldest sons; 19% (8) were youngest children, and 16% (7) were only sons. Overall, 31% of the loyalists were middle children, while 69% were oldest, youngest, or only children; the corresponding figures for the revolutionaries were 28% and 72%. In other words, middle children are underrepresented among our leaders; oldest and youngest children are overrepresented. Although our sample is both small and unrepresentative, these findings stand in sharp contrast to those presented by Sulloway. (The data in this paragraph are from Rejai and Phillips, 1988:48.)

In any event—and more importantly—we came to the realization that birth order is only one of an extremely large number of variables in the study of leaders. In particular, at least three groups of dynamics emerged, the first of which includes birth order:

- sociodemographic variables—age, birthplace, social class, education, occupation, father's occupation, ethnicity, religion, quality of family life, political culture and cosmopolitanism, political and military activity;

- psychological variables—patriotism and nationalism, vanity and egotism, compulsion to excel, asceticism, estheticism, relative deprivation, status inconsistency, marginality;
- situational variables—national crisis and emergency, violent culture, imperialist and colonial movements, family tradition, luck or chance.

In short, a vast array of forces and dynamics converge to shape leader personality. One hazards the proposition that no single variable is sufficient to explain the formation of all leader attributes. Nor does one anticipate an invariant mix of dynamics universally applicable to all leaders. A mix there shall be, to be sure, but one expects it to vary from leader to leader.

It is all too evident that Sulloway drastically and recklessly oversimplifies.

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The Birth Order Trap

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Born to Rebel (Sulloway, 1996) embeds a theory of birth order within an evolutionary perspective that gives a refreshing new twist to theorizing about family structure. Most past birth order "theorizing" is of the type that both begins and ends with "Isn't it interesting that" In Rodgers and Thompson (1985/86), we documented birth order research on presidents and strippers, artists and assassins, hockey players and cigarette smokers; even the birth order of birth order researchers has been investigated. Sulloway (1996), in contrast, provided explicit mechanisms to explain why he believes that birth order plays an important role in revolutionary behavior. The mechanisms derive from sophisticated thinking about both evolution and family dynamics.

But thinking that is refreshing and sophisticated is not necessarily correct, when held up against empirical evidence that provides the appropriate arena in which to test

social science theorizing. Sulloway (1996) argues unequivocally that his empirical evidence strongly supports the theory. Townsend (this issue) concludes just as unequivocally that Sulloway's data are nonsupportive of his theory. As in most such arguments, the truth probably lies somewhere between the two extremes.

The Birth Order Trap

I cannot be sanguine about even elegantly stated birth order theory. Both the public and social scientists have been much too willing in the past to believe that birth order explanations are rather more powerful than they really are. A number of sophisticated social scientists have been caught in the "birth order trap." This trap is sprung on social scientists when an interesting and plausible theory is developed, acclaimed, and widely accepted before the appropriate empirical tests are run to evaluate the theory. Zajonc's confluence model (Zajonc, 1976) and Blake's dilution theory (Blake, 1981) are two well-known examples. Rodgers (2001) and Rodgers et al. (2000) give my perspectives on how and why these and other birth order theories have failed.

However, relating birth order to social conservatism may be a more fruitful enterprise than developing birth order theories of intelligence or general personality factors. Sulloway's theory is a restatement and elaboration of an earlier birth order theory, the "conservators of tradition" hypothesis (see Edwards and Klemmack, 1973; Kammeyer, 1966; Komarovskiy, 1953; Sutton-Smith, Roberts, and Rosenberg, 1964). Mixed support was found for the earlier version of the theory. But, as I will argue in the last section, the appropriate data to test the theory have not been proffered. Whether the theory in *Born to Rebel* is supported by the data is still an open question, and neither Sulloway (1996) nor Townsend provide resolution.

Peer Review and *Born to Rebel*

Townsend's criticisms probably hold *Born to Rebel* to inappropriate standards, since the scientific thinking in that work was never held to the a priori standards to which most scientific work is held. Sulloway could have taken the traditional publishing model used by virtually all social science researchers. During the many years in which he was developing and writing his book, he could have instead published theoretical and empirical articles in social science journals. Each would have been carefully peer reviewed. Reviewers would have inspected the data sources, critiqued the analytic methods, and recommended relevant work of others of which he was not aware. Editors would have supervised an interactive process in which each article was criticized, revised, and re-criticized until the editor and reviewers were satisfied with the result. Subsequent

writing would have been informed by comments on earlier writing. Within *Born to Rebel* are results that might have been published in a dozen or so journal articles, implying that this work missed out on the critical scrutiny of around 30-50 anonymous reviewers, chosen for their specific expertise, and working for editors instead of the author (providing a very open forum for criticism). Ultimately, the reviewers and a few dozen other specialists would have read his articles carefully, a few hundred other scholars would have read his abstract and, perhaps, parts of the introduction and conclusions.

Instead, Sulloway wrote a book, published for and marketed to the general public. His book has been bought by thousands and thousands of readers, and studied by some unknown subset of those purchasers. But nowhere did the thinking, analysis, or writing pass through the traditional peer review system. Books, of course, go through their own review process, but with different goals and different standards. Book reviewers focus more on the big picture and on marketability than on details and scientific rigor. And this book turned out to be well marketed, widely read, and highly acclaimed in the (highly educated) public arena.

And have no doubt—the public loves to believe in birth order. I have given dozens of talks on birth order to students of all ages, to civic clubs, and to church groups. I receive several requests each month for interviews from journalists writing popular articles about birth order. I have listened to dozens of personal stories—most of which imply some simple birth order theory—about the relative intelligence, shyness, or athletic ability of Aunt Millie and her sisters or Grandpa Fred and his five boys, all of which boil down to the simple belief that birth order can explain almost anything.

Professional social scientists have been equally enthusiastic. Beginning with Adler (1928), and continuing with Thurstone and Jenkins (1929), Koch (1954), Zajonc (1976), Blake (1981), and literally hundreds of other qualified and well-known social scientists, birth order has been invoked as an important explanatory variable in models of child behavior and development. So it is not surprising that *Born to Rebel* sold a lot of copies.

It is also not surprising that, as Townsend documents in repeated detail, some of the standards that journals have for scientific articles were not upheld in this book. Even if Sulloway himself holds rigorous scientific standards—and I am firmly convinced from both professional and personal contact that he does—the peer review process administers a type of rigor that an individual, even with input from supportive colleagues, simply cannot achieve.

Sulloway's best defense against criticism as deep and sustaining as that in Townsend's article is to simply admit that the scientific standards of the audience for which his book was written were different and less rigorous than the ones Townsend (and professional social scientists) hold.

An audience that has never heard of Rubin's imputation methods can hardly critique or criticize their use in "fixing" the missing data problem. Nor does that audience expect the kind of detailed explanation that Townsend found lacking in descriptions of variables entered into multiple regression models. Nor can that audience properly evaluate what information and data sources should be included so that others can replicate analyses.

But Does the Theory Fit the Data?

If *Born to Rebel* could not and did not have the same standards of scientific rigor to which Townsend is holding it, Townsend nevertheless raises the very proper question of whether Sulloway's theory is supported by empirical evidence. Townsend's criticisms were careful and painstaking. But we need to see Sulloway's explanation for why he made the decisions that Townsend criticizes. If his work had been peer reviewed, exactly the types of criticisms that Townsend presented would have come forth—but behind the scenes, and prior to publication. Sulloway's risk in writing a long book instead of a series of journal articles is that fatal flaws early in the process cannot be caught and fixed as they can in the course of publishing a series of journal articles. Can Sulloway properly refute Townsend's criticisms? The real evaluation of the legitimacy of Townsend's claims will begin when we get to read Sulloway's response. But I will conclude by identifying a crucial problem only hinted at in Townsend's critique.

The Importance of Within-Family Data

In the remainder of this commentary I will present my own agenda, which I will lay down as "ground rules" for those engaging in birth order research. My opinion is that hardly anyone in the 125 years since Galton (1874) signaled our ongoing fascination with birth order has been careful enough methodologically. Ernst and Angst (1983) were correct to worry about controlling for sibship size and social status, and both Sulloway (1996) and Townsend follow. Kammeyer (1967), Schooler (1972), Adams (1972), Schvaneveldt and Ihinger (1979), and Rodgers and Thompson (1986) all presented methodological discussion of how birth order research should be conducted, and identified a number of pitfalls. But the most fundamental problem—the most subtle and devious of the many forms of the "birth order trap"—has only been given incomplete attention.

The starting point for birth order research must be the use of within-family data, in which siblings are compared, one to another, from the same family. A number of such studies have been run, but using within-family data has typically been treated as methodological innovation rather than as prerequisite. The basis for this position is that selection factors are both rampant and subtle in cross-sectional birth

order data. In a cross section, a firstborn black upper-class urban female may be compared to a second-born Hispanic middle-class suburban male and to a third-born white lower-class rural female. When differences are found, they may be attributed to birth order, to urban status, to gender, to race, to social class, or to many other explanatory factors that covary with these. When birth order research emerges from cross-sectional data—as almost all of it has—the choice of which explanatory factor is the real cause is an arbitrary choice. When patterns are viewed within families, however, such confounds are approximately controlled. Other methods exist to control for these types of selection factors as well, including difference models (e.g., Guo and VanWey, 1999) or instrumental variable approaches.

The thousands of past birth order studies are virtually silent with respect to the question they wish to answer, because virtually all have been based on patterns in cross-sectional data (see elaboration and documentation in Rodgers, 2001, and Rodgers et al., 2000). This is also true of the work in *Born to Rebel*, with the exception of the excellent and interesting sample of brothers (Sulloway, 1996:51). Sulloway agrees: "The acid test of birth-order effects is whether they occur among siblings who have grown up together within the same family" (1996:50). Townsend documents that Ernst and Angst made exactly this same point, concluding that "whenever studies of sibs within the same sibship were carried out, no differences in personality or personality traits associated with birth order were found" (Ernst and Angst, 1983:186). Rodgers et al. (2000) reviewed within-family studies of intelligence, and concluded that the strong birth order patterns that consistently show up in cross-sectional birth order data disappear when within-family patterns are inspected.

It is important to note that simply controlling with linear statistical models for sibship size and social status (and other factors) is not even close to methodologically adequate. First, these controls are typically only linear controls (although quadratic or cubic models can certainly be fit). Second, the models themselves are linear models. Even using interactive effects does not control for nonlinear dynamic process. Rodgers, Rowe, and Buster (1998), among others, made the point that most of the world is nonlinear and dynamic, while most of our models are linear and static. Third, and most important, such controls only account for the variables that are measured and modeled. The dozens or thousands of other possible confounding variables—many of which we might not even think of—are not accounted for. But within-family studies, or difference models (using longitudinal data), or instrumental variable approaches at least partially control for both known and unknown biases. These methods are not as effective as randomization for ruling out threats to internal validity, but they are highly effective in settings in which random assignment is impractical or impossible. Obviously, siblings

cannot be randomly assigned to different levels of birth order.

The implication of this position is that the argument over whether Sulloway's primarily cross-sectional data are properly compiled and listed becomes moot if the data cannot support the inferences that we wish to draw from them. Indeed, in a footnote in *Born to Rebel*, Sulloway noted that "birth order is not the *real* cause of radical thinking, even though it is strongly correlated with it. But birth order can be seen as a proxy for differences in age, size, power, and status within the family. Common sense tells us that causation probably lies in these other variables, not in birth order per se" (1996:373). The four potential confounds he lists explicitly are among thousands. Only when efforts are made to use sophisticated controls for both observed and unobserved selection factors can light be shed on whether the patterns his theory predicts are those that actually obtain.

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Birth Order and Personality: Is Sulloway's Treatment a Radical Rebellion or Is He Preserving the Status Quo?

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Franks Sulloway (1996) has stirred up great debate with his thesis that birth order explains and predicts people's proclivity to rebel. Scientists, scholars, business writers, and the broad reading and talk-television public alike all have weighed in to discuss the value and veridicality of Sulloway's thesis. Though many think he may be on to something, certain voices—Frederic Townsend's included—argue that his opus is simply bunk.

Yet, controversy hasn't seemed to hurt Sulloway's popular acclaim much at all! According to *Forbes* magazine, Sulloway's *several* symposia at the 1997 World Economic Forum in Davos, Switzerland—an annual intellectual retreat for the elite from across the public and private sec-

tors—had to turn away dozens of interested applicants. And his 1996 book, *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives*, was in its third printing just two weeks after it first came out, and in September of 1997, it was still on “top ten” lists of best-selling paperbacks in nonfiction. Though it may not be tenure, Sulloway’s controversial work has earned him an exceptional amount of attention across a wide swatch of readership, and over an exceptionally long period of time.

So what’s the beef about birth order? As Townsend explains in this journal, the notion that the order of a child’s birth among her siblings is important to her development is one that has spurred generations—literally—of behavioral scientists to scrutinize the family in an untiring effort to isolate causal forces associated with sibship and personality. As Townsend reflects, much of that effort has gone unrewarded. In their 1983 book reviewing over 1,000 pertinent studies, for example, Ernst and Angst concluded that birth order should be disaffirmed as a predictor for personality and behavior (quoted in Townsend, this issue).

Despite literature to the contrary, 35 years of research and observation by myself and my colleagues tells me that siblings have a profound and lasting influence on one another’s personalities. Like others, I was inspired by Helen Koch’s seminal work in the 1950s to explore in greater detail the influences of birth order on personality development (Koch, 1954, 1955, 1960). Her numerous tables provided a rich harvest indeed, and when Sutton-Smith and I labored through the morass, we found more than we had bargained for (see Rosenberg, 1982; Rosenberg and Sutton-Smith, 1961, 1968, 1969, 1974; Sutton-Smith and Rosenberg, 1969, 1970a, 1970b). We identified a number of factors that we termed “sibling,” “sex,” and “status,” and we included these along with birth order in a multivariate model that predicted an impressive proportion of variance in the complex phenomena we call “personality.”

Our research over those years convinced us that parents do, in fact, treat their firstborn children differently than they do their laterborn children, though the patterns are not simple and most definitely depend on such mitigating factors as the sex of the child, the sex of the child’s siblings, the age-spacing between children, and the family’s overall size and socioeconomic standing. Moreover, we now know we must add to the mix of experiential variables we discussed decades ago the still poorly understood influence of genetics, experience in utero, temperament, cultural context, and perhaps much more.

My position, therefore, is not to argue that birth order alone accounts for a significant portion of one’s adult personality. Rather, I believe birth order is a proxy for a host of exogenous influences that shape a child’s behavioral style. Not unlike Sulloway, actually, I subscribe to a contextual model of sibling influence. In this view, parent training—through means formal or informal—is limited in our society, and so trial-and-error learning is typical for par-

ents of firstborn children. Every burp and sneeze is greeted with anxiety and trepidation, as if the child might surely die! In this fashion, the firstborn child enjoys the unlimited attention of her parents, as she is the center of the family’s universe, at least for some time.

With the appearance of another new baby, however, the older sibling increasingly must share parental resources and, over time, must come to do things for herself. Eventually, modeling adult behavior emerges as an effective adaptation to these contingencies. Not surprisingly, parents may appreciate their oldest child’s attempts at imitation, and many will be pleased by their regard. More respectful of authority, more responsible in imitation of their mom or dad, and—perhaps—more inclined to support and preserve the status quo (who would care to repeat the trauma of that original displacement!), older siblings may, as Sulloway asserts, become strong yet conservative leaders as they emerge into adulthood.

In comparison to the firstborn’s identification with and modeling of parental figures, laterborn children are the beneficiaries of the trial-and-error learning their parents already have accrued. Subjected to less parental anxiety and inexperience, and so allowed more freedom to take risks and to try new ways, laterborn children emerge as more psychologically differentiated, more prone to obtain satisfaction by manipulation and harassment, more likely to take risks, less inclined to experience guilt, and less likely to appeal to authority, all relative to their older siblings. Relatively lacking in power, more subtle and devious means are necessary for the younger child to get his or her way. And where firstborn children have no consistently present model except that of their parents to emulate, younger children may develop more flexible patterns of identifying and adopting the very different behaviors of their older siblings or of adults as circumstances allow and demand.

According to Boynton—quoted from Townsend here—Sulloway says his theory “has the explanatory power that psychoanalysts always dreamed of having” (1996:81). No doubt, he is a bold man. In fact, psychoanalysis has realized that precision in predicting or explaining personality simply is not achieved, and so practitioners relegated the need for such rigor to a secondary place in their art. Most of us continue to believe childhood experiences within the family have a pervasive and formative effect on adult personality. Most major theories of development of the past 80 years attest to this truth. And while siblings play a major role in shaping one another’s behavior, it is infernally difficult to trace the threads of their influence—empirically, if not clinically—into adulthood. A generic personality trait in childhood may be transformed into a very different style or set of characteristics in adulthood, though they be of the same stem. Thus, the evolution of generic traits unfolds, becoming highly nuanced by the forces of nurture, and even submerging entirely over time.

Returning from theories of human development to the immediate task at hand, I am reminded of the old joke about Freud, which says “Much of what Freud said was new, and much of what he said was true; unfortunately, that which he said that was new was not true, and that which he said that was true was not new.” Unoriginal though I might be, I believe the same can be said of Sulloway’s arguments about birth order and sibling rivalry. That is, while Sulloway does an elegant job of conveying his story of the impact of birth on a personality trait he calls “rebelliousness,” the theoretical and mechanistic constructs he invokes to explain his findings are not new. Though he dresses his ideas in the Darwinian cloak of “competition,” Sulloway’s arguments about birth order actually recycle the classical constructs of Freud, of Bronfenbrenner, of Dunn and Plomin (Dunn and Kendrick, 1980, 1982; Dunn and Plomin, 1990; Plomin, 1994), and of so many others who came before him. In short—and to simplify his case—Sulloway tells us that firstborn children identify more strongly with their parents than do their younger siblings; that child development is best viewed from an ecosystems approach; and that intra-familial dynamics drive siblings to adopt different styles of accomplishing things in their lives. These concepts are not new. Hardly!

In closing, let us not forget that Sulloway is not a psychologist. He is an historian, and as such, his approach is not empirical. Rather, his methods of collecting and testing data are retrospective; the individuals in his sample—by virtue of their reflection in historical records—are not representative; and his “experiment” cannot be replicated. On this level, it is very easy to argue against *Born to Rebel*, and Townsend (along with several other critics he cites here) does this devastatingly well. One must be clear, though, that Townsend’s best case is an argument not with Sulloway’s conclusions but rather with the methods that the former MacArthur fellow employs. While the debate about the impact and importance of birth order may rage on, the crux of the argument here is about whether Sulloway’s use of highly selective, historical data is appropriate for inductive science.

Personally, I believe Sulloway’s treatment of birth order and sibling rivalry is quite clever; even brilliant. I also feel his basic findings may be true. Where more traditional, empirical approaches have failed to demonstrate what many parents—and even some serious psychologists—believe about the child, Sulloway has placed his finger on the pulse of a pervasive folk theory of child development, and in so doing he has persuaded several eminent scholars to testify to his genius. Perhaps it is the art in Sulloway’s work—rather than the science—that does this trick. Perhaps his is just the right mix of elegance, fact, and drama. Or maybe, *Born to Rebel* is just a penetrating glimpse into the obvious. Whatever his alchemy—and despite Townsend’s devastating assessment of the author’s methodological rigor—I

think Sulloway is a damned good read. I even hope the man enjoys his celebrity. In my opinion, Sulloway does psychology a service by instigating interest in the social and behavioral sciences. We do ourselves a disservice by taking him too seriously.

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Statistical Correlations, Nomothetic Principles, and Exceptions to the Rule

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At the outset I should admit that I am more positively disposed toward Sulloway's (1996) magnum opus than is the author of the target article (see Simonton, 1997). My positive disposition stems from three sources—substantive, theoretical, and methodological. Concerning the first source, I am more receptive to the notion that birth order may bear some relation to various forms of exceptional achievement. Galton (1874) was the first behavioral scientist to report such an empirical linkage, and his findings have been replicated and extended many times (Simonton, 1994). Indeed, I am among those who have discovered some of the relationships.

In the area of American politics, for example, I have shown how birth order is associated with certain characteristics of both U.S. Presidents (Simonton, 1988) and their First Ladies (Simonton, 1996). On the theoretical side, my own thinking, like Sulloway's, is explicitly Darwinist, and I have even endeavored to place Sulloway's model in a more comprehensive Darwinian framework (Simonton, 1999). Finally, with regard to methodology, I tend to consider Sulloway something of a comrade in arms. For the past quarter century I have focused my research program on the historiometric analysis of eminent figures (Simonton, 1990). In particular, I have applied sophisticated quantita-

tive analyses to biographical and historical data in order to understand the basis of exceptional creativity and leadership. Of course, this same method underlies the original studies Sulloway reported in *Born to Rebel*.

The above threefold admission is not equivalent to an assertion that I agree with all substantive, theoretical, and methodological aspects of Sulloway's book. I have my own set of quibbles and reservations. Nonetheless, those complaints are not the same as Townsend's, nor would I express them in the same manner as Townsend does. However, I leave to Sulloway the task of defending himself against the various attacks. Instead of undertaking that responsibility, I want to focus on just one issue raised by the article: the scientific validity of generalizations that are abstracted from historical data using quantitative techniques. Townsend's critique of Sulloway's historical studies devotes much time to discussing "exceptions to the rule." It is my view that any inventory of such exceptions is irrelevant to judging the scientific truth or falsity of a given nomothetic hypothesis. After all, it is virtually certain that such "laws of history," like virtually all behavioral laws, must be probabilistic rather than deterministic. As such, the nomothetic principles are expressed in terms of statistical correlations among variables. Unless these associations are perfect, there necessarily must be many exceptions to the rule.

Let me give an illustration from my extensive research on the predictors of presidential leadership. The assessed greatness of U.S. chief executives correlates very highly with number of years served in the White House (i.e., r 's in the .40s to upper .50s, depending on the performance measures used; see, e.g., Simonton, 1986). Yet that does not mean that no exceptions exist. On the contrary, the exceptions to this statistical regularity are quite conspicuous. Lincoln, for example, is much more highly rated than could be predicted according to tenure duration alone, and by the same predictor Grant is greatly underrated. These exceptions notwithstanding, it remains true that the longer a president serves in office, the higher his historical reputation is likely to be.

What the exceptions *do* reveal, however, is that historical phenomena usually have multiple determinants, so that any single correlation will have many exceptions that indicate the operation of these other determinants. In the case of presidential leadership, there are at least five other variables that provide reliable predictors of greatness assess-

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ments (Simonton, 1988, 1991). For example, Grant's low standing is partly explained by the scandals that plagued his administration, for Nixon and Harding exhibit a similar penalty. On the other hand, Lincoln's higher-than-expected standing can be partly explicated by his having been assassinated, a boost that is also apparent in presidents who suffered an equally tragic misfortune. The impact of assassination is especially provocative, because this factor does not have a statistically significant correlation with presidential greatness. Its predictive utility only emerges after being placed within a multiple regression equation in which the effects of the other predictors are mathematically controlled—an instance of what is known as “statistical suppression.” The rationale for this suppression effect should be clear. On the average, assassinated presidents serve less long than do the others, which means that they are penalized by the strong positive association between tenure duration and assessed performance. Only by holding years in office constant can the true consequence of assassination materialize. Needless to say, to the extent that suppression effects appear in a causal model, the more meaningless will be the simple inspection of supposed exceptions to the rule.

In any case, once all predictors are placed in a single multiple regression equation, we can account for over 80% of a president's greatness evaluation. Yet even that degree of predictive success does not imply that there exist no exceptions to statistical expectations. For instance, Washington's rating is far higher than predicted, a boost that most likely reflects his unique status as the nation's first chief executive (see Simonton, 1998). Only when we possess a prediction equation that explains 100% of the variance would exceptions vanish altogether. Despite the examination of hundreds of potential predictors, that degree of predictive precision has proven elusive (Simonton, 1981, 1987b).

Actually, the research on presidential greatness may not provide the best way to illustrate the general nature of statistical generalizations abstracted from historical data. Here the prediction equation explains a significant proportion of the variance using only a handful of variables, all of which function according to simple additive and linear functions. This outcome is highly atypical of most historiometric findings (for an account for why presidential greatness is so different, see Simonton, 1987b). On the contrary, most historical phenomena are so complex that the predictive accuracy is normally much less, even though (1) a much larger number of variables are incorporated, and (2) these variables are specified to operate according to curvilinear and multiplicative functions (for examples regarding a great diversity of achievement domains, see Simonton, 1976, 1980a, 1980b, 1984a, 1984b, 1987a). As an immediate consequence, exceptions to any simple (linear and bivariate) rule become much more conspicuous, and the ability

to decipher the basis for the exceptions far more precarious.

Naturally, because Sulloway cited specific cases in support of his generalizations, Townsend might argue that he has the right to present counter-examples. But it is one thing to mention a case that can be said to illustrate a statistical law abstracted from numerous cases, quite another to disprove that law by listing exceptions. The only way of achieving the latter end is to conduct a bona fide historiometric analysis.

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Born to Rebel: The Science of Birth Order

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In accordance with the principle of full disclosure, we believe we should inform the reader about several matters. First, as associate book review editors of the *Journal of Social and Evolutionary Systems*, we recommended for publication (but did *not* commission) a review by Frederic Townsend of *Born to Rebel* (Townsend, 1997). That recommendation was accepted, and the review appeared in the journal's Volume 2, Issue 2 in 1997.

Second, displeased by the review, Frank Sulloway wrote a lengthy and detailed letter challenging the accuracy of many of Townsend's criticisms. In the interest of fairness, we recommended that Sulloway's letter be published by the journal. That, we believe, will shortly be done.

Third, we (Alan Arwine, Steven Peterson, and Albert Somit) spent several years studying the possible relationship between birth order and political behavior. Our find-

ings were published in perhaps a dozen articles and, ultimately, as a book (Somit, Arwine, and Peterson, 1996). Both our data, and the conclusions we drew from those data, were strikingly different from those presented in *Born to Rebel*. So, too, we should add, were the sales: *Born to Rebel* (Sulloway, 1996) sold in the tens of thousands; the existence of our volume, we are obliged to report, has remained a well-kept secret. Indeed, if one looks at Amazon.com's sales figures (as of December 31, 1998), the paperback version of *Born to Rebel* ranked 6,068 among the three million titles Amazon was selling; our softcover book rated 331,771. In hardcover sales, *Born to Rebel* ranked 32,623; our hardcover version stood at 1,279,732. In addition, eighteen readers had reviewed Sulloway's book at Amazon.com; no one had reviewed our volume.

This information duly presented to the reader, we can now turn to Townsend's comments on *Born to Rebel*, generally, and his analysis of Sulloway's research, in particular. Townsend, to put it conservatively, takes exception to much of Sulloway's data and to his mode of statistical analysis, alleging serious errors of both omission and commission. Other contributors to this discussion will, we are sure, deal expertly with some of the technical issues involved; accordingly, we will focus, first, on the disparity between Sulloway's findings and our own; then, second, as does Townsend, on the troublesome elasticity of Sulloway's definitions.

As those who have read his book will recall, Sulloway argues three theses: first, and most broadly, that birth order affects behavior in many respects; second, that it affects and shapes *political* behavior; and third, and most specifically, that there is a significant relationship between birth order (especially being lastborn) and a proclivity to engage in acts of political rebellion.

Does birth order influence political behavior generally? In our research, we looked at American presidents, Supreme Court justices, congressmen, and stretching the definition a bit, American generals. Then, to see if there might be a different pattern elsewhere, we examined British prime ministers, UN secretaries-general, the Soviet leadership, and, another sizable stretch, "great generals" in history. In none of these instances could we establish a statistically significant (.05) correlation between birth order and political behavior (i.e., defined as success in achieving office and/or behavior while in office). No matter what measure we applied, we could find no statistically significant dif-

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ference between firstborns, second borns, third borns, etc., etc.

No, that is not entirely correct: one small qualification is in order. Where the offices were elective (or required a special education, i.e., as for judicial position), firstborns tended to do better than predicted on purely demographic (i.e., incidence of firstborns in the population) grounds. The reasons are quite simple: (1) having a college education has been, and remains, almost a prerequisite for many elective offices; and (2) as is well known, until quite recently firstborns (and males in particular) were more likely to have the advantage of a higher education than their laterborn siblings. However, once we controlled for education, the differences normally evanesced.

Since the political offices we studied were predominantly held by males both in the United States and abroad, the next question was whether birth order played any significant role in political achievement and behavior among *women*? Here, of course, to get a satisfactory “*N*” we were compelled to limit our inquiry to U.S. state and local offices. But the same pattern emerged again: once we controlled for education, birth order was simply not a factor (see Somit, Arwine, and Peterson, 1997).

The foregoing relates, of course, to the evidence dealing with the purported influence of birth order on political behavior *generally*. For the reasons stated above, we are skeptical of any such claim and would have to agree with Townsend that *Born to Rebel* does not present really convincing evidence of such a relationship.

Nonetheless, this still leaves open the logical possibility that birth order (and especially being a lastborn) might be a factor in terms of special modes of political behavior—such as acts of rebellion. And that, to be sure, is Sulloway’s major contention. Despite the criticisms that some have raised against this volume, we do recognize the immense effort that went into the data gathering and analysis. *Born to Rebel* clearly stands as one of the more rigorous volumes on birth order; as Ernst and Angst (1983) long ago pointed out, much of the research on birth order is, candidly, rubbish. Sulloway has raised the endeavor distinctly above the norm, no mean feat. While we are critical of his work, we think that this needs to be acknowledged.

Admittedly, of course, we ourselves have not developed any data directly challenging this claim. We did not do so, however, because we could not arrive at a definition of “re-

billion” which, on the one hand, was reasonably intellectually rigorous and which, on the other, was sufficiently broad to generate an “*N*” large enough to permit satisfactory statistical testing. It is precisely on this point, we feel, that Townsend’s strictures are most valid. He challenges the validity of the data on which Sulloway bases his conclusions. Also, and we see this a major and legitimate concern, he points out that Sulloway’s definition of rebellion encompasses such disparate modes of behavior as intellectual/ religious/ scientific/ and literary innovation, self-report on some attitudinal scale, and voting for the execution of Louis XVI, to mention only some of the items of inclusion.

In short, we would have to agree with Townsend’s basic contention. *Born to Rebel*, we believe, does not make a convincing case for either the larger proposition (birth order influences political behavior) or the more restricted version thereof (birth order is significantly correlated with acts of political rebellion). With regard to the former, there is a substantial body of contrary evidence that Sulloway essentially ignores; with regard to the latter, we can only say that, as yet, there has been no truly persuasive evidence presented, one way or the other.

While we are duly impressed by the database developed by Sulloway, we are not yet persuaded that he has overcome the previous body of evidence that, at best, is inconclusive in demonstrating a key role for birth order in explaining human political and social behavior. While we are not able to judge the specific points raised by Townsend, we do think that the colloquy in these pages advances discourse on the study of birth order and politics in a more substantive manner than has often been the case. This, in itself, is a major contribution of this set of commentaries.

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Resolving Controversy over Birth Order and Personality: By Debate or by Design?

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The notion that birth order bears a systematic relation to personality was commonly thought to have succumbed to the formidable attack from Ernst and Angst (1983). The 1996 publication of Frank Sulloway's book *Born to Rebel*, however, breathed new life into a proposition that most social scientists thought was a dead horse. In response to the book, Townsend (this issue) insists that Sulloway's extraordinary measures to revive this dead horse were unwarranted. Apparently fearing that too many readers have taken the book seriously, Townsend attacks both the contemporary and historical evidence offered by Sulloway.

We do have a modicum of sympathy for Townsend's frustration with the presentation in Sulloway's book, *Born to Rebel*. A scientific treatise should permit a skeptical scientist to reconstruct the data used to justify its claims. Townsend found himself unable to fully reconstruct the statistics supporting several of Sulloway's central claims. If not absolutely exhaustive, however, Sulloway's documentation was certainly prodigious. It doesn't seem fair to attack a book on the basis that it did not provide the same level of detail as an empirical article. Further clarification

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of the archival evidence is certainly necessary, but Sulloway has apparently already begun that process by amplifying the documentation in his follow-up publications (Sulloway, 1999; in press).

Townsend also found Sulloway's conclusions to be extravagant. We concur that the presentational style is immoderate at times. We see little fault, however, because Sulloway was aiming at lay readers as well as social scientists. In light of this intention, Townsend's response to the book must be characterized as—at best—an over-reaction. His reply is intemperate in its absolute dismissal of Sulloway's findings. And throughout, Townsend repeatedly impugns Sulloway's integrity. Our own reading of *Born to Rebel* revealed a literary style designed to captivate and persuade a wide audience—not to mislead them.

The Complexity of Rebelliousness

One of the strengths of *Born to Rebel*, in our view, is the attempt to integrate birth order effects with those of other developmental variables. Indeed, the book makes it clear that birth order is only one of a number of interacting predictors of personality and social attitudes—none of which approach perfect prediction.

Townsend, however, portrays this approach as a cynical attempt to explain away inconsistencies—a litany of convenient qualifications that seem to be the crux of his discontent. Townsend's unabashed contempt for Sulloway serves to undermine the persuasiveness of his critique because the tone is overly zealous and, at times, insinuates impropriety.

For example, Townsend attacks Sulloway's depiction of personality correlates of rebelliousness, suggesting they are chosen to vilify first-borns: "...rebellious laterborns are unassertive, forgiving, submissive, and timid. Non-rebellious firstborns are violent, vengeful, jealous, and militant." It seems to us that Townsend is mistaken in his impression that Sulloway is uniformly negative about first-borns. Sulloway argued that later-born children adopt niches to reduce invidious comparison with older, larger, and more mature siblings in order to maximize investment from parents. The resulting pattern of personality variables follows in a straightforward fashion. The assignment of traits to

first and laterborns is indeed discriminating, but not discriminatory: After all, Sulloway has argued in various reports that first-borns are more conscientious, more confident, more achieving, and more intelligent. Hardly uniformly negative.

We hasten to point out that, more than any other critic to date, Townsend has tried to review the original literature cited by Sulloway—a monumental task to be sure. The number of discrepancies between Townsend and Sulloway in factual matters is rather disturbing. Because we have not directly consulted the original articles (before Ernst and Angst), we will not enter that debate, but focus on what our own research can contribute to the larger picture.

Newer Data

Our more sanguine view of Sulloway's efforts ensues in part from the fact that our own research has largely sustained Sulloway's claims. We did not set out with that goal in mind; nor did we have any personal contact with Sulloway at that time. Our analysis of *Born to Rebel* suggested that within-family comparisons were the method-of-choice for uncovering birth order effects. With that perspective, we have collected more than a dozen large samples of individuals asked to compare themselves and their siblings on various personality and achievement dimensions. The results have since been presented in print and conference presentations (Paulhus, Trapnell, and Chen, 1999; Paulhus, Trapnell, and Wehr, 1999; Wehr and Paulhus, 2000). We can only summarize them here.

In our *Psychological Science* article, we demonstrated birth order effects on personality and achievement in four large data sets including both student and adult samples (Paulhus, Trapnell, and Wehr, 1999). Control over a wide range of variables was effected by collecting within-family data: Participants were asked to compare their siblings (and themselves) on a variety of personality and achievement dimensions. Across four diverse data sets, later-borns were rated as more rebellious, liberal, and agreeable. First-borns were rated as more conscientious and achieving. The same results were obtained in both adults and students in two countries. The same results were obtained whether or not birth order was made salient (to activate stereotypes) during the personality ratings. In short, these large-sample, highly-controlled studies supported predictions from Sulloway's niche-model of personality development as well as the earlier confluence model of intellectual achievement (Zajonc and Markus, 1975).

In our follow-up research, we compared within-family and between-family data on the same research participants. The within-family design yielded birth order effects between two and three times stronger than those obtained from between-family designs (Paulhus, Trapnell, and Wehr, 1999; Wehr and Paulhus, 2000).

We are convinced, therefore, that birth order effects on personality can be obtained by empirical designs with sufficient power. At the same time, we have acknowledged the relative difficulty of showing birth order effects in between-family data.

Conclusion

The history of science has shown that critics demand more proof for revolutionary claims than for those that confirm the current beliefs. Indeed, this persuasion difference applies to everyday audiences as well as scientific ones (Hovland, 1966). From this perspective, Townsend's reaction to Sulloway's efforts is not unexpected and is likely representative of many commentators. After all, Sulloway's claims are radical for their time, and skepticism is natural. If it were framed as a plea for further documentation and more data, then, Townsend's critique could be seen as a useful contribution to the literature. Unfortunately, the tone is marred by innuendo.

Because he is not touting a competing theory, one might conclude that Townsend is just a hard-nosed, demanding skeptic in his evaluation of all new ideas. Perhaps we are biased and should be more critical of Sulloway. If so, it is a bias induced by seeing a clear and consistent pattern in our own samples of within-family data. Almost invariably, the patterns support Sulloway's predictions.

Continued debate about the archival data may be futile because of the frustrating variability in quality and methodology of the original studies. Collection of new, carefully-targeted data will be more compelling, because Sulloway's hypotheses can be tested more directly. And recent methodological improvements (e.g., design controls, consensually-accepted measures of the Big Five traits) can be more fully exploited.

In particular, new research must address the fact that birth order effects appear more clearly with some experimental designs than with others. It is not surprising that effects should be more clear in within-family than between-family designs: The former usually have more statistical power to find real effects. When the recent between-family designs do reach significance, they tend to support Sulloway (Davis, 1994; Trapnell, 1995; Wehr and Paulhus, 2000). Other within-family studies have supported Sulloway's predictions (Salmon and Daly, 1998; Sulloway, in press). Even the recent report by Jefferson, Herbst, and McCrae (1998)—cited as a refutation of Sulloway—actually shows mixed support. The latter authors puzzled over the paradox of why peer ratings supported Sulloway's findings whereas spouse ratings did not.

Even if the within-family effects are substantiated, a reasonable rejoinder is that within-family differences may not generalize to personality outside of the family setting (Ernst and Angst, 1983; Jefferson, Herbst, and McCrae, 1998;

Harris, 1998). We suspect that there is much more to be learned about this provocative issue.

We conclude by noting the consensus emerging in behavioral genetics that (1) between-family differences in personality and intellect are dominated by genetic variance, and (2) the environmental variance is mostly within-family (e.g., Bouchard, 1997; Dunn and Plomin, 1990). Therefore, social scientists interested in ameliorative interventions must turn their attention to the latter. Genetic variance must be acknowledged before we can address environmental theories, including family-dynamics. Otherwise, any single within-family source such as birth order or idiosyncratic peer influence (Harris, 1998) may seem insignificant. Yet, even modest effect sizes can translate into dramatic social consequences (Rosenthal and Rosnow, 1991; Sulloway, in press). And it is precisely the within-family effects that will be most responsive to the benevolent potential of social science.

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