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LINGUISTICS AS A GENETIC SCIENCE*

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I am designedly using the term 'genetic' rather than 'historical' in this context because, after having been for thirty years a devotee of the discipline conventionally labeled 'historical linguistics', I have at present grave misgivings about the unqualified suitability of the label and, far more important, I have become alert to the implications of its possible inappropriateness.

To begin with, the label 'history', like many terms of ancient scholarship, is fraught with imprecision. It refers to an analytical discipline concerned with the study of the past, but also with a segment of that past regardless of any analysis, as long as there exist any written records. Only thus can we explain the fact that prehistory designates, at least in normal usage, another, earlier segment along the time axis, whereas such terms as prelinguistics (which may or may not have been coined) and prephilology (which has actually been toyed with on one occasion) could meaningfully refer only to a prescientific stage of a discipline; that is to say, to cognition itself, not to an object of cognition.

My second source of doubt is the awareness that any serious work of history involves a number of commitments on the part of the practitioner not only as to the purpose, scope, and techniques of his research, but also as to the forces credited with causing change. This hard core of curiosity about causation in general, with special reference to evolution, must, of course, be shared not only by straight historians, i.e. students of broadly significant events in the succession of human societies, but also by scholars combining a special topical expertise with a flair for sequential reconstruction—say, art historians or historically oriented analysts of jurisprudence. To my surprise, I discovered before long that, despite the familiarity of many old-style linguists with an astonishing profusion of historical details, very few among them operated explicitly with any clearly delineated philosophy of history. True, Émile Littré, the late-19th-century author of a massive dictionary of Modern French, had, at the outset, a generous share of close contacts with Auguste Comte's positivism; also, Otto Jespersen's disputed optimistic belief in the steady improvement of language as a tool—witness his book Efficiency in linguistic change—implied a sharply silhouetted world view, as one would expect of the rationalistic author of a publication entitled The philosophy of grammar. But, generally speaking, among the older linguists addicted to the historical approach, particularly those immersed in etymology, it is easier to find individuals with a strong side-interest in various facets of material civilization, in navigation and caravan routes, and even in botany (cf. Vittorio Bertoldi), ichthyology (witness Paul Barbier), and ornithology (I refer you to Richard Riegler) than persons with any pronounced

* Substantially unaltered text (except for the added excursus on primary vs. secondary determinants) of the Presidential Address delivered to the Linguistic Society of America, in Chicago, on 28 December 1965.
flair for, let alone systematic training in, etiology—the name under which the
theory of causation is known in philosophical circles.

The third doubt that has assailed me relates to a curious paradox which bids
us revert to the aforementioned distinction between history and prehistory.
Students of evolutionary linguistics are in the habit of complaining almost
ritualistically about lacunae in their information, but, strangely, such problems
as suffer from few gaps offer them no real challenge and are thus unlikely to
attract many first-rate minds. There exists almost an excess of data on the transi-
tion of written Latin from the Golden Age to the Silver Age, and the socio-
cultural background, for once, is amenable to minute inspection; but such a topic,
for all its philological legitimacy, falls flat among red-blooded linguists, as would
the study—equally defensible yet equally unexciting—of, say, English terms of
sports and athletics recently absorbed by miscellaneous modern languages. It is
prehistory, with its conjectures so rich in hazards, that titillates the daring
linguist’s imagination; and this proclivity again makes the term ‘historical
linguistics’ something of a misnomer.

A fourth idiosyncrasy of evolutionary research in linguistics must now claim
our attention. Peculiar to the data-gathering process in historiography is the
gradual concentration on details and a concomitant attitude of tactical meticul-
osity which measurably slows down strategic progress and therefore makes genuine
breakthroughs increasingly rare. Through a cruel irony, it is the honest, patient,
scrupulous, erudite worker who is most inhibited by his knowledge of residual
gaps in the information garnered and by his excruciating awareness of any
severe limitations of his personal competence. Leaving aside such talented
amateurs as H. G. Wells and cynics whom I prefer to leave unmentioned, the
rare professional historian attempting a bold synthesis based on a new theory of
cycles—I have in mind men as gifted and hard-working as Spengler and Toynbee
—must reckon with an investment of thirty-odd years of sustained effort and
unstinted labor, i.e. practically a lifetime spent on a single project. The rank and
file of workers infected with historical curiosity, however, shy more and more
away from such audaciously architectured edifices and prefer to earn polite
recognition if not enthusiastic acclaim through rigorous inquiries into paro-
chially delimited problems. Thus there unfolds before our eyes a world of shrink-
ing horizons, in which monumental structures are allowed to crumble, and only
compact, tightly built cabins seem safe enough to survive. I submit that this
kind of attitude of apprehension, carried to an extreme, can have a stifling effect
on evolutionary linguistics and that, to avert lasting harm, we must counter-
balance the ever-present temptation of détaillisme by carving out, from the over-
flowing mass of raw data and of minute accurate findings, such salient discoveries
as lend themselves to wider generalization and higher abstraction, thus constitut-
ing a welcome feedback into the main stream of general linguistics. It is on this
battleground, throughout the last half-century, that describivists of many
hues have had all the advantages, and evolutionists, in the guise of historians,
all the drawbacks. Historical linguistics, to be sure, has its continued justification
for specific painstaking probings into unique societal settings—a slant of study
to whose charm, for well-known sentimental reasons, most 19th-century scholars
succeeded and not a few of their 20th-century successors continue to respond, and which has, I am confident, a future. But to ensure that future, which presupposes increased maneuverability, we must learn to isolate a core of theory and exemplification—if necessary, at the cost of studied restraint and even of painful surgery—and establish a discipline concerned with the universals, as against the particulars, of change. Whether we decide to call it glottodynamics, or evolutionary linguistics, or genetic linguistics, is a matter of expediency and esthetic preference. I realize that in life sciences, genetics, concerned with neatly foreseeable hereditary features, and evolution, focusing on mutations far more difficult of prediction, are mutually opposed—in some quarters, almost to the point of irreconcilability; but this contrast seems not to apply to the growth of language. There need occur no divorce from conventional historical linguistics—in some instances, only the friendliest of separations, with visitation hours, will suffice; in others, still better, the alternate performance, by the same versatile worker, of both assignments will maintain a most desirable liaison, as when Émile Benveniste cultivates the two perspectives, before our eyes, in an elegant zigzag movement.

One supervenient reason for favoring ‘genetic’ over ‘historical’ is that the latter label applies, by definition, to the past, whereas the former, in Janus-like fashion, points to events both behind us and ahead of us. This statement implies no simplistic belief in any easy predictability of approaching trends of developments bearing on language. Unlike his naturalistic counterpart, the linguistic geneticist will perhaps be well-advised to curb his optimism, confining himself to the hope that from the observation of the past and of the present he can try to extrapolate one ingredient or streak of the foreseeable evolution, an ingredient whose effects can be reinforced, weakened, neutralized, or in extreme cases even reversed by other elements either utterly imponderable at this state of knowledge or, at best, tentatively assessable from the vantage points of disciplines other than the study of language. Even if that much ground has been gained, the genetically oriented linguist, unlike the straight historian and quite unlike the orthodox descriptivist among his confrères, may well cease to protest, in an almost morbid recoil from personal involvement in reality, that he is eager to observe, to record, and to analyze, but not, under any circumstances, to influence events. Perhaps we can transcend this long-dominant attitude of impossibility bordering on passivity by arguing that at the moment of actual dissection the analyst should in fact declare himself totally uncommitted but that, at the immediately following stage of evaluation, he is perfectly free not only to draw forceful inferences as to certain factors emerging as potentially influential, but also, if circumstances so warrant, even to throw his weight behind shifts and movements deemed desirable—if not qua linguist stricto sensu, at least qua linguistically enlightened and chastened layman.

The substance of what I am propounding (as against certain emphases placed conceivably for the first time) is by no means new, and the very word ‘genetic’ was sporadically applied to linguistic science by some mid-19th-century workers—witness the *Vocabolario genetico-etimologico della lingua italiana* by G. B. Bolza, traceable to the year 1852. More important, the implications of this
approach were felt on both sides of the Atlantic throughout the decades separat-
ing, on the one hand, the concluding phase of W. von Humboldt's activities and
their immediate posthumous reverberations and, on the other, the temporary
consolidation of neogrammatical theories and practices around the mid-eighties.
A. Schleicher's organic view of the development of language has been attrib-
uted, in conventional retrospect, to the vogue of Darwinism. In more careful re-
appraisals, it has recently been observed that crude or refined evolutionism, by
the mid-19th-century, was somehow in the air, nourishing initially both Darwin's
biological inquiries and Schleicher's parallel thinking along a less sensational
line. In the final elaboration of his technique and terminology, Schleicher could,
of course, have freely profited from the powerful impact of such universally in-
fluential works as *The origin of species by natural selection* (1859); he did not
live to see the publication of *The descent of man* (1871).

As happens not infrequently when scholars toy with the wisdom of a fresh
start or grope for new shapes of lingering ideas, those of us eager to see diachronic
linguistics rehabilitated must, among other things, strive to reassess some earlier
judgments which, by dint of repetition, have become rigidified but not necessarily
unassailable. If, in casting a glance at 19th-century linguistics, we allow our-
selves to be guided exclusively by Holger Pedersen's well-known book, readily
available in a tasteful English translation, we may easily obtain a skewed, grossly
distorted picture. Pedersen, a seasoned Celticist and an expert Indo-Europeanist,
was swayed in his retrospective value-judgments by what—on the scale of the
early twenties—seemed most beneficial for certain types of Indo-Europeanist
reconstruction; though he exceeded the Indo-European domain in sheer scope
of coverage, he fell short of transcending it in methodological preference. Through
a queer and inexcusable caprice, he also abandoned a solid Danish tradition
stretching from Vilhelm Thomsen to Viggo Brøndal and beyond in completely
slighting Romance linguistics. By dividing all of 19th-century scholarship into
two parts and by assigning to the first the experimental writings of a Bopp, a
Rask, and a Schleicher, while reserving for the latter the technically far superior,
but in other respects distinctly narrower analyses of the leading neogrammarians,
he conjured up an image of progress which, I think, can and should be chal-
lenged unless one implicitly agrees to equate certain favored chapters of IE
historical grammar with linguistics as a whole. For while there indisputably
occurred a change around 1870, that change carried with it not only an undeniable
increase in precision and refinement, but also numerous retraclements and a
withering of curiosity along several once promising lines. With regard to liber-
alism, I am tempted to compare the third quarter of the 19th century in Europe
and, thanks to William Dwight Whitney, in New England to the intellectual
climate in this country throughout the twenties and especially the thirties, when
linguistic research, as symbolized by Edward Sapir's many-splendored and
imaginative if disciplined gropings, was carried out in freedom from the op-
pressive constraint so unpleasantly characteristic of the forties and the fifties.
It was only in the last quarter of the 19th century, which I venture to regard as
a period not only of triumphant advances but also of tacit retreats, that the
concept of historical linguistics, with its full scale of implications and connota-
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Linguistics, began to crowd out the concept of genetic linguistics. Perhaps the turning point, on the semi-popular level, was Hermann Paul's long-celebrated and tone-setting Prinzipien der Sprachgeschichte (1880; 2d enlarged ed., 1886).

What sets off, by no means disadvantageously, the pioneer geneticists or evolutionists from the bulk of 19th-century language historians is a set of qualities such as these: first, greater concern with, and keener understanding of, universals of linguistic change, sometimes achieved—let us candidly admit—at the cost of premature generalization; second, sustained curiosity about fluctuations of meaning, with no concurrent neglect of changes of form; third, superior open-mindedness toward non-Indo-European languages; fourth, full recognition of the importance of etymology, conceived not only as a purveyor of dependable equations for historical grammar, but as a discipline worth pursuing for its own sake; fifth, unabashed delight in modern dialect research and, coincident with this predisposition, growing finesse in the requisite open-air operations and in subsequent analyses of data so obtained. Let us exemplify some of these propensities and, in the process, observe how they mesh and intertwine.

For general linguistics the contemporaries of Whitney, moving in the groove traced out by Humboldt, used the term 'philosophy of language'. In calibrating the output of these trail-blazing scholars, one notices a balance between data-saturated monographic researches and methodologically slanted early syntheses, including the two major book ventures by Whitney himself, of the years 1867 and 1875, respectively. The accumulation of austerely monographic probings, unrelieved by experiments in theory and speculation, becomes, in contrast, characteristic of the concluding decades of the century. It may be argued, as I realize only too well, that in certain contexts premature syntheses are harmful; but that argument yields in persuasive power to the contention that the blunting and stunting of the explorer's imaginative drive may, in the last analysis, represent the greater evil. As a temporary moratorium, the exclusion of so exciting a topic as the origin of speech from the deliberations of a prestigious linguistic circle may be understood and condoned; but, in sober fact, the shelving of a problem of this magnitude and intensity of appeal marks not a victory, but the masked admission of a stunning defeat.

As regards heightened concern with meaning, Michel Bréal's Essai de sémantique inevitably comes to mind, a book whose publication date—1897—at first glance removes it from the early genetically biased period of linguistic science. But if one recalls that the author was born in 1832 and had published, by the early sixties, four monographs bearing on Zoroastrian religion, comparative mythology, and Persian anthroponymy, it is plausible to assume that the book's period of gestation pertains to a distinctly earlier, pre-neogrammatical period. Rather characteristically, Bréal's first translator and annotator, J. P. Postgate, confesses at the very outset of his lengthy Preface, dated 1900, that as a young man in 1877 he had selected the science of meaning for the subject of a 'Fellowship dissertation' at Trinity College, transforming that essay almost twenty years later, but before Bréal entered the arena of semantics, into an inaugural address at University College, London. The radiance surrounding the activities of a startlingly precocious Ferdinand de Saussure during his Parisian period
must not blind us to the fact that there were other remarkable French linguists at work in the capital—scholars unjustly eclipsed, whose belated rehabilitation must be based on the rôle they played as precursors of Bréal. I have especially in mind two experts whose names I rarely hear in linguistic quarters. One, already alluded to, was Littré, a companion-in-arms, you will recall, of the positivist Auguste Comte; his early writings include a tract on the electrodynamic theories of Ampère; closer to home, he authored a treatise entitled *Comment les mots changent de sens*, issued posthumously in 1888. The other was the talented Arsène Darmesteter, who in his highly readable Sorbonne lectures at the start of an all too short career, collected two years earlier than the date of the Littré pamphlet and published under the title *La vie des mots étudiée dans leurs significations*, included the most elaborate version of an ambitious ‘théorie du développement des sens par rayonnement et enchaînement’, clearly a kind of embryonic semantic wave theory.

For the mid-19th-century’s impressive ability to straddle IE and non-IE languages—that is to say, for the enthusiastic espousal of the platform of the trail-blazing typologist Wilhelm von Humboldt rather than the program of the early comparatists Bopp, Grimm, and Rask—I wish to adduce, as a clinching if slightly unexpected argument, the evidence of Romance scholarship. Between the founding father of Romance linguistics, Friedrich Diez—a counterpart, if you wish, of Jakob Grimm—and the vigorous systematizer of that discipline at the threshold of the 20th century, Meyer-Lübke—whose work you are free to view as a pendant of Brugmann’s—lies a hazily silhouetted territory dominated by two major figures, Graziadio Isaia Ascoli and Hugo Schuchardt.

Ascoli’s lasting merits lie in the domain of fine-grained Romance dialect studies, especially the monumental mosaic of Raeto-Romance which he pieced together in his *Saggi ladini* (1873) and his later characterization of another sharply profiled dialect cluster which he considered a separate Romance language—the torso of a less laborious monograph embodied in his *Schizzi franco-provenzali* (1878). But the theoretical conclusions which he drew from the minute inspection of medieval and modern dialects—namely the substratum theory (or, as some Italians call it, ‘dottrina delle reazioni allogene’) with special reference to Celtic, subjacent in certain territories to Latin and Romance—these important inferences, transcending his immediate purposes, he would hardly have reached without that independent acute curiosity about Celtic which prompted him, in 1878–79, to examine the Irish glosses of the Codex Ambrosianus. Celtic, again, was to him merely a major prong of his earlier broader advance in the direction of Indo-European, a preoccupation which yielded his well-known—and, I understand, not undisputed—inquiry into the velar series of the parent language, and which also cast off studies in Persian and in Gypsy; while the Indo-European family in all its richness acquired a sharpened contour within his private cosmos through confrontation with Semitic, a step made possible because Ascoli’s early contact with Hebrew had awakened in him to desire to become an Orientalist, bidding him to acquire the necessary tools. This is to say nothing of his incidental excursions into Turkish, Dravidian, and Chinese.

Similarly, Schuchardt, after taking as his starting point Vulgar Latin, and after
presenting the wave theory in a nutshell, with verbal and pictorial illustrations, nine years ahead of Johannes Schmidt's programmatic treatise, branched out from these modest beginnings on a scale unprecedented in the annals of Romance philology, not only appropriating, one by one, such adjacent domains as Slavic and Celtic, but also developing an astounding competence—reminiscent of Trubetzkoy's and Sapir's elasticity—in Basque and in paleo-Iberian; in Hungarian; in Georgian; in miscellaneous Creole languages—amalgams involving English, Spanish, and Portuguese, on the one hand, and West African, Indic, and Malayo-Polynesian tongues on the other (witness his nine sketches published in quick succession from 1882 to 1891); as a separate inquiry, in the speech of Saramakka Negroes transplanted to the Dutch colony of Surinam in the Northeast corner of South America; in Berber languages; and even venturing, ahead of Otto Jespersen, into the realm of artificial languages such as Volapük (1888).

On a less ambitious scale and without Schuchardt's unique intuitive flair, a scholar almost forty years his senior—in fact, distinctly older than Ascoli—also reconciled his active layman's curiosity about English and Romance dialects with heightened concern about Basque, not entirely dilettantic: I refer here to Prince Louis-Lucien Bonaparte (1813-91), a nephew of the Emperor, who, despite his modest rank as a scientist, deserves mention hic et nunc because his incomparable collection of early dialect monographs—a veritable treasure trove—was acquired quite some time ago by Chicago's Newberry Library, which is within a stone's throw from the place where we are assembled.

Over against this slant of performance, Meyer-Lübke, typifying to some extent neogrammatical preferences and standards of craftsmanship at the close of the century, displayed commendable industry, competence, and honesty in turning out, again and again, either historical grammars or articles conducive to such grammars; in so doing he seldom abandoned the sheltered Romance hunting ground and practically never mustered sufficient audacity to strike out beyond Indo-European.

My fourth point, the uninhibited enjoyment of etymology for its own sake rather than merely as an ancillary operation, stands somewhat apart. Undeniably the merit and sheer necessity of sound etymological analysis were quite unapologetically recognized by the pioneers. The emerging custom called either for the same scholar to produce, as mutually complementary repositories of knowledge, a grammar and a dictionary, in launching a two-pronged attack on a language or a family of languages, as practiced by Grimm and Diez; or for one expert's grammatical edifice to flank another's string of etymological vignettes, witness the experiments of Bopp alongside those of Pott. I can think of no plea in defense of etymology more eloquent than Whitney's in the concluding chapter of his book *The life and growth of language*; let me quote from this passage just a few lines:

The whole process of linguistic research begins in and depends upon etymology, the tracing-out of the histories of individual words and elements. From words the investigation rises higher, to classes, to parts of speech, to whole languages. On accuracy in etymological processes, then, depends the success of the whole; and the perfecting of the methods of etymologizing is what especially distinguishes the new linguistic science from the old.
So far Whitney; his forceful commitment in this matter clashes pathetically with Saussure's and Bloomfield's baneful inarticulateness. It is true that the summit of etymological escalation was reached only several decades later, at the turn of the century, after the first flowering of dialect geography and the crystallization of the Wörter-und-Sachen approach spearheaded by Meringer and Schuchardt. But it is equally true that the then novel techniques which gave such powerful impetus to 20th-century etymology were nourished by the spadework of free-wheeling mid-19th-century evolutionists (or geneticists) far more than they were by the gradual retrenchments of neogrammarians, for all the superiority of the latter group's organization.

If one were to press into a single formula the chief services that a grand strategy of lexicology, including its etymological kernel, renders to linguistics, beyond its humble illustrative and exemplifying function, I am tempted to propose that it be recognized, first, as the main artery of communication with the ceaselessly changing hard facts of the outside world and, second, as a safety valve for the study of individual, especially of abnormal, growth—for matters, that is, which stringent grammarians eager to concentrate on patterns tend, for the sake of economy or tightness, to sweep under the rug. Yet even the most austere, grammatically oriented linguist is unlikely to deny that erratic configurations sporadically scattered over the surface may conceal invaluable clues to older and even pristine patterns, partially overlaid. Thus any prolonged withdrawal from etymology, under whatever pretext, threatens to produce the blocking of vital supply lines and, in the end, the total inanition of historical linguistics.

The fifth and last salient trait of the trail-blazing researches, before the various lines of curiosity hardened into a fixed hierarchy, was the workers' active concern with dialect speech. Of course the records prepared were clumsy, the transcription used inadequate, the underlying theoretical assumptions—if any—often naive; but openmindedness was clearly in evidence. The later, far more elaborate dialect monographs and the currently accepted canons of fieldwork have their ultimate roots, I believe, in the mentality or, if you wish, attitudes of the scholars toiling approximately a century ago.

It is not my intention here, let me emphasize, to extoll the somewhat dimmed merits of one generation of linguists and of their lineal descendants, pushed into the background, while belittling (or attempting to cut down to size) the accomplishments of a more aggressive subsequent generation. The real point at issue is quite different and has distinctly greater bearing on some of today's events, both exciting and disquieting. When speaking of progress in linguistics, we can use this term with unalloyed pride only where one technique supersedes another without leaving any residue. To provide a handy frame of reference: the jet-plane passenger service from San Francisco to Chicago marks an improvement over the older propeller-plane service because it indisputably accomplishes the same clearly circumscribed task, transporting an individual and his luggage from one city to another, under measurably more favorable conditions, as regards time, boarding procedures, seating, comfort, protection from noise, processing of checked suitcases, and en route entertainment—visual and gustatory. In other words, there remains in commercial aviation virtually no residue of
pleasurable or useful experiences lost in the course of planned technological
replacement. The situation is less clear-cut where ferryboats have been super-
seded by automobile traffic over bridges, because in that context the strictly
utilitarian and the esthetic or sentimental benefits seem to be at loggerheads. In
the annals of linguistics there may have prevailed some smooth transitions,
without concomitant losses. But even the improvement which for years passed
off as the smoothest of all—the substitution of phonemic for phonetic descriptive
models—left a heavy residue; and the two shifts from early evolutionism,
characteristic of the 1860's, to the strict historicism of the neogrammarians and
later to ironclad structuralism entailed, counter to widespread belief, spectacular
retreats as well as advances. In mapping out the future of our discipline, it is
entirely legitimate to inventory the losses suffered at earlier junctures and easily
overlooked in the flush of triumph, so as to reabsorb into the mainstream of
linguistics any valuable ingredient that may have gone astray.

Before veering away toward the present and the future, let us briefly examine
some terminological innovations launched by the early evolutionists and their
direct successors. The term substratum—in French, alternately substratum
and substrat before the fairly recent standardization of the latter—was reportedly
coined in the mid-18th century, to denote a philosophical concept, and it emerged
again, after a certain lapse or a period of intermittent use, apparently—in 1876,
on the pages of the Parisian Revue des deux mondes, then a tone-setting journal
read all over Europe. The application of the term to linguistic layering dates in
France from 1882 and, in all likelihood, presupposes intervenient specialization
in the direction of geology; I gather that Italy, through Ascoli as a spokesman
for its semi-autonomous tradition of glottologia, for once preceded France in the
coinage of an unequivocal designation of the subjacent speech layer. 'Substra-
tum', by virtue of its prefix, invited an unambiguously polarized name for the
reverse situation, a 'crust' overlaying the principal deposit, and W. von Wart-
burg, in the early thirties, provided an answer to this need by advocating the
label superstratum, which would apply, for instance, to Merovingian Frankish
superimposed on Gallo-Romance, to Maghrebi Arabic partially superjaent
vis-à-vis Hispano-Romance, to an unidentified variety of South Slavic—possibly
akin to Macedonian—stretching over Daco-Romance and permeating it with its
ingredients, and to Mexican Spanish forming a roof over Maya in the Yucatan
Peninsula. 'Substratum' and 'superstratum' admirably fulfilled an actual de-
mand of stratigraphic linguistics—a discipline we must now carefully distinguish
from stratificational analysis—and have lately enjoyed unqualified recognition
on both sides of the Atlantic. I am uncertain as to which 20th-century expert,
from the ranks of Romanists, presumably, in fact launched adstratum, but I
find the word far richer in noteworthy potentialities than the inconclusive uses
so far essayed would lead one to aver. If we are to believe J. Marouzeau's ter-
minological glossary (Lexique ... 3; Paris, 1951), 'superstratum' and 'adstratum'
have of late been employed interchangeably, a situation which, once proven true,
would call for the elimination of the less widely adopted member of this set of
doublets. Salvatore Battaglia, in the opening volume (1961) of his Grande
dizionario, maintains that 'adstratum' involves a state of balance or equality
between two layers, the implication being, it seems, that substratum, in contrast, suggests not only anteriority visually dramatized by subjacency, but also hierarchical inferiority; the 'adstratum', to quote Battaglia, 'indica il contatto fra due lingue di cultura che si influenzano reciprocamente senza però che si abbia la prevalenza netta dell’una sull’altra'. Would this apply to a social cleavage like the one between aristocratic Anglo-Norman and the native rustic speech that one is led to interpose between Old English and Middle English? Or should we look to countries like Belgium and Switzerland for exemplification? I, for one, am tempted to reserve 'adstratum' for a rarer, yet more neatly delimitable situation, as when nuclei of Greek and Osco-Umbrian speakers spread, carrying their languages with them, over the western Mediterranean under the aegis of the rapidly expanding, culturally liberal Roman Republic. By the same token, 'adstratum', adjusted to the context of modern immigration, could aptly label certain admixtures of Italian and Galician-Portuguese speech to the uninter ruptedly predominant layer of Spanish in the La Plata zone (Buenos Aires and Montevideo); also, under comparable conditions, it might point to recently identified elements of Italian, Yiddish, and Caribbean Spanish in the racy speech of New York City and other metropolitan centers of this country abounding in newcomers from abroad. Of course, the coinage of neologisms in -stratum can become as much an idle parlor game as was, for two long decades, the derivation—half-serious, half-jocular—of qualifiers in -emic ('sememic', 'tonemic', 'stylemic', etc.) and -fix (witness 'interfix', 'coaffix', 'superfix', 'transfix'); small wonder, then, that Carlo Battisti recently overreached himself by experimenting with the ugly hybrid parastrato in Chapter IV of his Sostrati e parastrati nell' Italia preistorica. On the other hand, I heartily applaud Italian linguists for freely experimenting with the ending -oid: suffixoid is crisper than 'quasi-suffix', Liguroid is most helpful to convey the idea that a language spoken outside Liguria was genetically or typologically akin to Ligurian proper. To revert to geology, a useful borrowing from its lexical inventory has been un filone 'streak, vein (in the earth)' for all kinds of minor but far-flung admixtures, a metaphor favored by some Italian scholars, e.g. Vittorio Bertoldi, and occasionally by their Spanish counterparts (una veta); sediment and deposit have been welcomed in many quarters; so have been such key words as residual and vestigial.

Another developmental science with which linguistic evolutionists established an early rapport was paleontology. The term 'linguistic paleontology' itself enjoyed but a short vogue; I can trace it to the three editions of Meyer-Lübke's classic Einführung in das Studium der romanischen Sprachwissenschaft (the first, of 1900; the third, of 1920); did the author borrow it from Ascoli? The neologism failed to strike root in linguistic quarters; but a few satellite formations, including 'petrifact', 'fossilization', and 'ossification', have acquired citizenship rights, whereas 'skeletal outline', propelled by some descriptivists, pertains to a different metaphoric strain.

Medicine, though not a genetic science, is so closely interwoven with biology as to have benefited from this tide; while the old-fashioned arbitrary concept of 'decay, corruption', as laid down by dogmatic preceptivists, was gradually yielding ground to more sober and more scientifically definable notions, part of
this archaic imagery was salvaged and allowed to fuse with the new terminology spreading from medical schools. Gilliéron's 'pathology' and 'therapeutics' have left a profound, indelible impact on genetically oriented lexicologists; to this day they are certainly more defensible, if one agrees to make allowances for teleological concepts, than the same scholar's later, inexusably flamboyant catchwords such as 'bankruptcy' (1919), 'hypnotism', 'phantasmagoria' (1921), and even 'thaumaturgy' (1923). But though Gilliéron, not least through his overstatements and his flair for theatricals, drove a wedge for this biological approach and its terminological trappings, it was a scholar less given to sensationalism, namely Littré, who as early as 1880 included in a by and large forgotten miscellany of articles one piece entitled 'Pathologie verbale, ou lésions de certains mots dans le cours de l'usage'. Darmesteter's oft-cited La vie des mots provided the bridge between the writings of the two men: Birth, life, death, evolution, transformation, struggle, health, creativity, stagnation, attrition remain the slogans from cover to cover, and, in perceptive anticipation of Gilliéron's probings into the outlandish and the erratic, one stumbles here over a reference to 'teratology', i.e. the study of monstrosities. Long before Darmesteter, Whitney, endowed with almost uncanny foresight, warned of the potentially perilous inroads of physical sciences and of psychology on the domain of linguistics; but, as is shown by the very title of his 'outline', The life and growth of language, he readily came to terms with the life sciences.

But enough of the past. This retrospect, which is not meant to sound a nostalgic note, will have served its limited purpose if it has aroused your curiosity (as it has my own) as to what the most imaginative geologists, paleontologists, and paleobotanists may have done in recent decades and, in fact, may be accomplishing at this very moment that should be worthy of at least some linguists' sustained or passing attention. To cite just one concrete instance of a blind alley into which linguists, by dint of gratuitously isolating themselves from other evolutionists, have run in the last quarter-century: The age-and-area hypothesis, i.e. the tentative reconstruction of a sequence of events from the present territorial distribution of a pair of erstwhile rivals, has been inconclusively eyed by a handful of linguists with an occasional furtive side-glance at sociology. But, surely, at least some geologists and paleontologists must have reached noteworthy conclusions as to the cogency of such inferences from space to time in their own disciplines. I should very much like to see the problems of spatio-temporal linguistics (as I think this branch deserves to be called) reopened for vigorous discussion, on condition that the participants agree first to take cognizance of any parallel progress recently achieved by standard-setting naturalists.

Finally, if I may venture a very ingenuous remark on botany, I have followed with a layman's admiration the skill with which naturalists manage to narrate the biography of a tree from a closely reasoned analysis of its rings. Thus, if the innermost rings are relatively broad and evenly spaced, the analyst infers that at first the tree grew with no disturbance. Subsequent rings of the same tree may show, through the varying impairment of their earlier breadth and evenness, that something pushed against it, making it lean (the rings are then wider on the opposite side); that the crowns and root systems of the tree's neighbors deprived
it of much of the requisite water and sunshine; that, after the harvesting of surrounding trees, there was once again ample nourishment and adequate exposure to sunlight; that a fire swept through the forest, perhaps only scarring the tree; that a prolonged dry spell once again slowed the tree’s growth; that some insect (like the larva of the sawfly), by eating leaves and leafbuds, may have adversely affected the health of our much-troubled tree. This mosaic-like reconstruction, under optimal conditions, of the probable sequence of events can never be duplicated by linguists, simply because languages do not cast off, at regular seasonal intervals, crusts or barks hardening into rings or shells; but a good deal can be observed and assimilated, I surmise, both from the heartening breakthroughs and the inevitable pitfalls of techniques adopted by favored neighboring disciplines.

There exist several other universals in linguistic reconstruction which cry out for separate treatment and may even demand the immediate remedy of existing practices. In the last thirty years not a few previously established sound shifts which long appeared unintegrated, dangling loosely in the air, so to speak, have acquired a new and more interesting perspective through the efforts of diachronic phonemicists. Scholars so oriented have often succeeded in placing isolated events in a meaningful chain or in establishing helpful cross-connections. In each instance the earlier description or attempt at explanation turned out to be not necessarily inaccurate and worthy of rejection, but rather unpalatably meager, if viewed in critical retrospect. In numerous cases the modern analyst’s ability to operate with entire systems rather than with individual sounds has indisputably had an eye-opening effect. The advance thus achieved has been from a survey of the surface to an inspection of the depth, if I may resort to two ultra-fashionable words ordinarily used in a different context. Yet even the most commendable idea or correlated technique has a limit; any indiscriminate application of the new insight beyond the saturation point threatens to produce rapidly diminishing returns. While diachronic phonemics retains its usefulness as a handy means of bracketing and underpinning an otherwise crumbling congeries of individual sound changes, it provides no answer to several legitimate questions of considerable weight, such as this one: Under what ensemble of circumstances can an aberrant morphological condition set in motion a drift of events eventually conducive to a regular sound change? We are so accustomed to seeing phonology take precedence over morphology, starting with the typical organization of introductory textbooks and monographic grammars, that any such morphological adjustment (e.g. leveling, diversification, wholesale replacement of suffixation by prefixation, etc.) as marks a speech community’s reaction to a preceding sound change accountable in strictly phonological terms strikes us as natural, while the presumably rarer reverse concatenation of cause and effect seems counter to expectation. Undeniably, one runs in one’s readings across parenthetic remarks to the effect that certain minor sound changes may have been stimulated by analogical regroupings in the inflectional, affixal, or compositional scaffold of the language under study. But a systematic inquiry into this category of possible, if infrequent, change—an investigation so ambitiously con-
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duced as to be cross-cultural, cross-spatial, and cross-temporal—remains, unless I am grossly misinformed, a mere desideratum.

I could easily here enumerate other problems of comparable magnitude which clamor for a resolute frontal attack rather than for evasive, inarticulate allusions. In order not to tax your patience, however, I shall concentrate, for the remainder of my Address, on just two, admittedly many-faceted issues: that of multiple (or complex) vs. single (or simple) causation, and that of primary vs. secondary determinants. This thread of discussion will lead us straight into the center of etiology and may, at the same time, add a much-needed touch of redeeming concreteness to this evening's deliberations.

There has been considerable progress in the recognition of different categories of change, especially within phonology: Thus, it would hardly occur to any Romance expert at present to champion the application of, say, 'Verner's Law'—which defines the impact of the word-stress on the outcome of certain consonants in paleo-Indo-European—to a sliver of material that does not fall under that 'law's' jurisdiction. This century's scholars, unlike some of even the keenest among their predecessors, have learned to distinguish quite sharply between universals and particulars. However, no major efforts have yet been made to explore the compatibilities or plausible successions of different determinants of change. Without any reliable guide to a searching analysis of the simultaneous impact of these affinities, complementarities, and reconcilabilities (or, conversely, to the patterns of their mutual exclusiveness), no serious study of complex causation can be undertaken. By the same token, the typical sequences of causes and effects in temporal projection and, within this schema, the hierarchy of major (or primary) and minor (or secondary) determinants deserves increased attention. To drive home my point, let me first succinctly restate, under a new angle, four problems of Romance linguistics, two of which have given rise to memorable discussions, while one—the first—has never been posed, let alone examined.¹ The kindred problem of primary vs. secondary determinants will, conversely, be thrown open in abstract terms, involving at most an imaginary situation in a nonexistent language.

Problem No. 1. There exists a sizable number of modern Spanish adjectives (most of them disyllabic) with a masculine singular ending in unstressed -io, e.g. agrio, amplio, lacio, limpio, lucio, medio. Is it warranted to posit a productive derivational model? To answer this question, we must first break down the twenty-odd formations collected into their historical sources. The diachronicist will distinguish between two major subgroups, of which the first includes adjectives, both vernacular and learned, whose segment -io could have been foreseen from elementary knowledge of phonology, while the second encompasses the more difficult and thought-provoking cases.

The first subgroup falls into three minor subdivisions: (a) vernacular descend-

¹ The following section of the Address embodies, in drastically compressed and partially rephrased fashion, the results of a monographic investigation ('Multiple versus simple causation in linguistic change') which is to appear in the new testimonial volume for Roman Jakobson planned by Mouton & Co. (The Hague).
ants of Latin -idus adjectives, e.g. flaccidu 'flabby' > lacio, limpido 'translucent' > limpio; (b) one vernacular descendant from a Latin -eus adjective, involving a radical-final bilabial consonant: rubeu 'red(dish)' > rubio 'blond'; (c) learned descendants—some of them very old—of Latin -ius adjectives, regardless of the radical-final consonant, e.g. mediu 'half' > medio, tertiu 'third' > tercio, also nesciu 'ignorant' > necio 'silly' and nimiu 'excessive' > nimio 'prolix, stingy'.

In the other subgroup, further categorization is also possible, but within every narrower category the marked tendency of each word history to yield a highly individual vignette is in evidence throughout.

In at least three cases the common denominator for the anomaly is the use of -io for expected *-o. The three adjectives at issue are soberbio 'proud', from superbu; agrio 'sour', from acriu beside acre; and amplio 'wide', from amplu. In the vernacular layer of Old Spanish, superbu should have cast off *sobiervo, while superbia 'pride' should have emerged as sobervia, and in fact did so. May one argue that the 'ideal' form *sobiervo 'proud', placed alongside the adjectival abstract sobervia, would have produced a morphophonemically unparalleled pair, an impending situation that has led to leveling in favor of the substantival stem variant? With agrio, which has superseded the perfectly regular OSp. agro, there is room for interpretive disagreement: Some scholars note the pressure of agriar 'to sour' (despite the late appearance of that verb) and adduce as parallel the equally startling transformation of ambru 'bitter' into Sp. amargo, doubtless through contamination with amargar 'to embitter' < amāricāre; others seek the source of associative interference in the widespread fluctuation between, say, ve-, vi-dro 'glass' < vitru and vidrio 'id.' < vitreu, lit. 'glassy'. To account for amplio, we may recall that amplius frequently furnished an equivalent of ultrā, praetereā, plus and, as a result of this extended use, gave rise, within the bounds of Latin, to the verb (ex)ampliare rather than *ampliare.

In another triad of formations, even more intricately structured, there has occurred an accent shift, from -io to '-io. The three adjectives in question are sandio 'foolish', za5io 'uncouth', and reacio 'obstinate'. In the old texts, the combined evidence of rhymes and meter shows that the word for 'foolish', etymologically opaque, was stressed sandio; cf. the continued comportment of its congener Ptg. sandeu. The Spanish word later fell into desuetude; when, after centuries of dormancy, it was revived, probably as a consequence of newly awakened interest in Cervantes' Don Quixote, readers identified it as scindio, conceivably on the analogy of its near-synonym nécio. Za5o is in all likelihood an Arabism, though Orientalists are in disagreement as to its specific model. The latest conjecture classes it as a cross of safhīn 'stupid, shameless' and safī 'pure'. Be that as it may, the earliest witness to its transfusion into Spanish, the Granadino convert Pedro de Alcalá, ca. 1500, placed a stress mark unequivocally on the i. Reacio 'stubborn', recorded in older texts as refazio, -hazio, is unmistakably an offshoot of OSp. refazer 'to do over and over again', from remodeled reficere, and involves the same derivational suffix -ivus as does OFr. restif; cf. Eng. restive and mod. Fr. rétif.
At the periphery of the lexicon one detects, particularly in dialects, a few isolated formations whose testimony carries much weight. Extr. *ludio* ‘leavened’ matches standard *leb-* > *leu-do*, from *LEVITU*, and may involve the avoidance through metathesis of a doomed falling diphthong in some such immediate prototype as *liudo*. The other item, apparently branching off directly from *pender* ‘to hang’, is *pen-* or *pin-dio* ‘bent, leaning’, peculiar to the north of the Peninsula.

If *agrío*, *amplío*, *ludio*, *pindio*, *reacio*, *sandio*, *soberbio*, and *zaño* exhibit a certain vitality of this type within Spanish, the scheme thus authenticated would raise the number of unstressed derivational suffixes once deemed unproductive in Romance.

We have thus far isolated the following causes, ascertainable or highly probable, behind the crystallization of -*io* adjectives: perpetuation of prototypes in -*IDUS* (*limpio*); limited survival, in the vernacular stratum, of bases in -*EUS* (*rubio*); adoption of learned formations now early (*nectio*), now late (*nimio*); influence of the co-existent abstract (*soberbio*); reverberations of the autonomous use of the Latin comparative (*amplio*, perhaps with the collateral support of the verb *ampliáart*); analogical extension of wavering between an old substantive and an erstwhile adjective tending to evict it (*ve- vi-dro ~ vidrio → agro ~ agrío*); accent shift in a word reintroduced into restricted use (*sandio*) or in one favored uninterruptedly (*reacio*); restructuring of a lexical item borrowed from an exotic language (*zaño*); and free-wheeling use of -*io* (*diálo. pendio*), sometimes coincident with metathesis (Extr. *ludio*). Each individual explanation, in some instances adduced to account for a single adjective, seems defensible; is any one powerful enough to have allowed an observer to predict, with assurance, this particular course of events?

Such a tangled situation provokes a number of interlocking questions. If halfway satisfactory conventional explanations are readily available for all or most of the items, why hypothesize a new derivational model such as radical-stressed adjectival -*io*? If, despite our misgivings, we accept this model as a contributing force, do the other explanations fall by the wayside, or can two (or more) causes be declared mutually complementary and co-efficient? Does the assumption of multiple causation clash with the principle of maximum economy as a yardstick of cogent scientific analysis, to the extent that such parsimoniousness is reconcilable with the complexity of the facts analyzed?

In seeking to provide answers, one must discriminate between mere description and causal explanation. When we state that *liudo* was locally transformed into *ludio* and list this process under the rubric metathesis, we simply attach a handy tag to a shift which, within that dialect, happens to be practically unpredictable.

Our second step is to remind ourselves that, even where probabilistic statements can be ventured, such assessments as involve a low degree of predictability prod researchers to seek for additional determinants. Assuming that a unique socio-historical situation involves a protracted state of bilingualism, thus favoring the surmise of substratum influence, surely the staunchest supporter of such hypotheses will grant that only selected features of sound structure, lexicon,
etc. are bound to filter through. Clearly certain asymmetries, lacunae, or ambiguities—in short, flaws in the architecture of the receiving language—may control the extent of assimilation of the ingredients adopted from the donor language.

Our third thought turns toward quantification. If, to simplify matters, we argue that the crystallization of all -io items except those extracted from Latin through normal channels involves the convergence of two forces—Force A, the agency of an apparently productive derivational model not yet fully identified, and Force B, some collateral pressure familiar from earlier inquiries—we observe that, in the slice of material examined, Force B varies from case to case: B₁ (accent shift), B₂ (adaptation of a loan), B₃ (contamination by a related verb), etc., while Force A, pervasively at work, ties together all these motley minor alterations. The positing of a heretofore unsuspected force—in this context, of an expanding derivational type—gains in plausibility if that force is each time paired off, in a series of complex mutually related processes, with a companion force of distinctly narrower scope.

One final consideration: Can the agency of the new factor be observed, at least occasionally, in isolation? For agrío, amplío, reacio, etc., at least one other driving force could be tentatively identified, with varying degrees of accuracy. The tidy isolation of even one clearcut instance would immeasurably bolster up any hypothesis reckoning with a new separate ingredient in multiple or complex causation.

**Problem No. 2.** This is a classic illustration of irreconcilable disagreement between two scholars of comparable caliber; it revolves around the alternation of [s] and [r] in Hispano-Romance, [s] being locally an allophonic member of /d/. Of the two disputants, E. G. Wahlgren (1930) brought to the arena a superior knowledge of cognate languages, his critic T. Navarro (1931) a more intimate glimpse of fluid dialect usage.

Wahlgren granted that in certain Romance languages the transformation of d into r could represent a regular sound shift, a kind of ‘rhotacism’. Elsewhere (e.g. in Spanish), he argued, the process was set in motion by a subtle interplay of (a) lexical or affixal analogies and (b) saltatory (sporadic) sound changes; but he apparently did not allow for any intermingling or alliance of these two major groups of causal ingredients. Thus, in dissecting Spanish words (predominantly of dialectal stock), he implicated lexical contamination, as with badajo ‘clapper (of a bell)’, changed into barajo allegedly through contact with baraja ‘pack (of playing cards)’; the intrusion of a derivational suffix, as in Ast. antröiru ‘carnival’ instead of expected antroidu, through pressure of -oiru, or in dial. párparo ‘eyelid’ < párpadolo, standard lámpara ‘lamp’ < LAMPADA, through association with ‘-aro, -ara; the interference of a co-existent infinitive, as in seguírilla beside older seguidilla ‘form of stanza’ and mentira ‘lie, falsehood’ beside Cat. mentida, through contamination with seguir ‘to follow’ and mentir ‘to be false’ respectively; and miscellaneous disturbances subsumable under assimilation (Cloro- < Clodomiro), dissimilation (paeres < parees < paredes ‘walls’), and metathesis (padéron < paredón ‘thick wall’). In a small residue of more complicated relationships, Wahlgren recognized a joint effect of some of the agencies.
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just mentioned, while steering clear of any assumption of regular phonological change; this more elastic attitude applied to regionalisms like arbolera 'grove', buirador 'brassworker', pelarela 'loss of hair', porvaera 'cloud of dust', and veder 'sidewalk'.

The vulnerable spot in Wahlgren's position is the extraordinary range of diverse phenomena, many of them of an optional rather than obligatory incidence, to which he resorts in an effort to elude any appeal to regular sound shift; one notes, as a result, the strikingly thin representation of each phenomenon in the inventory of determinants. Why not first deduct a few ancient cases (like lâmpara), which must obviously be fitted into a different context; then posit for the 20th century a state of widespread fluctuation [-s-]/[-r-], still bidirectional, but unequivocally pointing toward r as the ultimate victor, a situation recalling a sound shift in statu nascendi, i.e. one which may or may not eventuate, depending on the final outcome of the interplay of contending forces? If we agree to view these disturbances as mere symptoms or harbingers of a pending shift, does it not stand to reason that this preliminary scouting and groping—as speakers unconsciously feel their way toward a possible breakthrough which, once it is a fait accompli, linguists will dub a sound-shift—should be directed toward points of least resistance? These are just such points, in the edifice of language, as might cause the existing structure to crack through pressure independently applied by miscellaneous processes, such as metathesis, dissimilation, lexical or affixal analogy, etc. If these heterogeneous established causes effect, again and again, the same shift, one has every reason to suspect that there has been operative, initially in alliance with and through them, some new and sharply focused force pressing in a single direction.

Problem No. 3. The explicit marking of a given morphological category in excess of the traditional norm has been labeled hypercharacterization. Thus, Latin possessed the category of gender and frequently expressed relevant contrasts with maximal crispness and neatness: FIII-US, -A; BON-US, -A, -UM. In other contexts the gender was signaled less vigorously, cf. TRIST-IS, both masc. and fem., and VETUS, a single form shared by all three genders. From the parallel changes of GRANDIS to Fr. grand, -e, of VIRIDIS to Fr. vert, -e, and of a pair uncharacteristically marked by its endings, such as socer 'father-in-law' vs. socrus 'mother-in-law', to clearcut It. suocer-o, -a and Sp. suegr-o, -a, we can infer a protracted trend toward hypercharacterization of gender.

In a few instances this trend seems to be at work alone, as when OSp. cuchar (fem.) ‘spoon’ yielded to mod. cuchara. In most cases there occurred a splicing of this tendency with others, liable to separate isolation. Thus, if VETUS -ERIS, ailing from inadequate marking of gender (hypocharacterization), was replaced in some varieties of provincial Latin by VETULUS (cf. It. vecchio, Fr. vieil), in others by *VETERU distilled from VETERAN (OPtg. vedro, OSp. viedro), in yet others by a cross of VETULU and *VETERU (as is true, on phonological evidence, of Sp. viejo), then affixation and inflection—for one detail, the co-existence of PAUPER -ERTS and MISER -I ‘poor, wretched’—may also have shared heavily in the process; cf. Sp. pobre, alegre alongside It. povero, allegro.
In the domain of zoonymy, the marking of the animal’s sex through gender was at the outset limited to the barest minimum, except with certain domestic animals where the male and the female of the species provide radically different services (say, BÖS vs. VACCA). At the start, LEÔ ‘lion’ and LUPUS ‘wolf’ were consequently ambigeneric, MASCULUS and FÉMINA being added at intervals for incidental emphasis on the sex, much as are bull (or buck) and cow, cock and hen, he- and she- in English or, for that matter, -bock, -kuh, and -kalb in German. Only later did speakers coin LUPA for ‘she-wolf’, fig. ‘harlot’, and LEA for ‘lionness’.

Significantly, wherever lexical innovations struck root in Romance zoonymy, regardless of the specific justification of each replacement, one recurrent fringe benefit was the sharper delineation of the gender (and sex). Thus, for ambigeneric CAN-ÉS or -IS ‘dog’, Spanish displays an exotic substitute, perro, flanked by perra ‘bitch’; near-by Portuguese proved hospitable to cão < CANÉ, closely linked with cadela ‘bitch’, from CAT-UĻA, -ELLA, orig. ‘female puppy’. French offers a third solution, placing chienne alongside chien, much as, in a Frenchman’s world-view, a lionne shares the lion’s lair. The lexical and grammatical vicissitudes of the ‘cat’ were slightly different: The original designation FéLÉS was feminine, like UOLPÉS ‘fox’ and MÉLÉS ‘marten, badger’, and the reference was initially quite vague as regards both sex and species (‘wild cat’, ‘weasel’?). The introduction of GAUTUS (gattus), an invader not attested before the 4th century, in all likelihood signaled three events: the adoption of a new label, the importation of a hitherto unknown species, and the acceptance of a novel social institution (domestication of the mouse-chasing pet); but—more vital to us—it coincidentally provided a welcome means whereby speakers could instantly tell a male from a female. The descendants of LEPUS ‘hare’ invite similar analysis, and one could almost indefinitely expand the roster of illustrative examples. Every single elaborative substitute may be amply justified, I repeat, in terms of such trivial phenomena as lexical borrowing, orchestrational attachment of augmentative or diminutive suffixes, analogical innovation, etc. But if the student of causation succeeds in isolating one consistently emergent by-product of all these multifarious processes—namely the more explicit marking of gender and sex—he is entitled to vindicate for the agency effecting the change the status of an autonomous codeterminant, because this force brackets untold developments doomed otherwise to appear disparate, meagerly exemplified, and almost random.

Problem No. 4. Consonant dissimilation at a distance has been a notorious crux of explicative linguistics. The event that best qualifies as the starting point for the peak of the debate is the publication of M. Grammont’s doctoral thesis (1895). That monograph attempted to elevate the chosen class of dissimilation to the rank of a set of stringently formulated sound laws, cross-temporal and supposedly binding on all speech communities. Grammont’s book, and a major complementary article from his pen, provoked a flurry of formal reviews and lively prises de position ranging from glowing or lukewarm approval to flat rejection and involving such experts as Brugmann, Meillet, Niedermann, and Schuchardt, to cite just a few names. Gauchat, likening the saltatory change at
issue to mutations in botany, warned against indiscriminate use of the facile label (‘moyen commode d’explication’). Fresh vigor was injected into the discussion by Rebeccia Posner’s Oxford dissertation (Consonantal dissimilation in the Romance languages, 1961), which had the merit of eliciting a bold counter-proposal from K. Togeby only two years later. Like Grammont, Posner believes in the survival of the fittest as the overriding principle; but she skilfully nuances her predecessor’s teachings by heeding phonemics, observing the incidence of sounds, spelling out explicit conditions for regressive effects, separating the central from the peripheral sections of the lexicon, and distinguishing, especially as regards the sequence of two r’s, between attitudes of individual languages.

In his sparklingly phrased counter-theory (‘Qu’est que la dissimilation?’, Rom. Philol. 17.642–67, 1964), Togeby admits that consonant dissimilation occasionally operates with the precision of a genuine ‘sound law’, e.g. where the succession of labiovelars is the stumbling block, as in quinque. For the most part, however, it makes its appearance sporadically, a situation which forces the analyst to use the term, at best, as a descriptive tag (‘étiquette sans valeur’) and, in fact, urges its elimination from any causal argument (650). The core of Togeby’s own reflections resides in his repeated insistence on the dispensability of any assumption of a consonant’s dissipilatory change or fall if some other explanation, equally plausible, can be proffered. A generous string of such persuasive rival explanations is next displayed in searching detail, the possibilities varying from a medieval scribe’s lapsus calami and an early etymologist’s faulty base to lexical borrowing, hyperurbanism, onomatopoeia and expressivity, crosses within the same word family, blends with some other word family, folk etymology, suffix or prefix change, agglutination and deglutination of the article. What adds a touch of drama to these comments, aside from the author’s polemic mastery and technical versatility, is to see a scholar reared in the tradition of radical structuralism and, to this day, steeped in it, side for once with such champions of the opposite extreme as Schuchardt and Gilliéron.

Despite my admiration for Togeby’s spirited advocacy, let me provisionally state this in partial criticism of his contention: From the fact that a phenomenon, while definitely identifiable, is seldom found in isolation, I incline to infer that this isolable phenomenon usually operates in conjunction with others. Once the common occurrence of multiple causation is granted axiomatically, there will be no need to proceed to radical extirpation of any suspected redundancy.

To cite concrete instances, Togeby is at his most seductive when he corrects the geographic trajectories of certain words traced hazily by Posner, as when he argues that it is not Fr. caramel which must be confronted with calamellu, but the French word’s immediate prototype, Sp. caramel ‘lollypop’. In other cases, possibly more numerous, Togeby’s alternative hypotheses lead to richer orchestration of the earlier analyses rather than to their complete abandonment. Thus, in almost-learned Sp. plegaria ‘prayer’ < precaria the dissimilation invoked by Grammont seems to be the decisive force behind the withdrawal of the first r in favor of l, but plegar ‘to fold’ < plícäre may have been, on the semantic side, a contributing factor (by suggesting genuflection). Furthermore, the switch in Spanish from an older, rustic form pregar ‘to fold’ to a socially more
elegant variant *plegar* created an ideal setting for the simultaneous replacement of *pregaria* by *plegaria*. Our attention, then, is drawn to an ensemble of circumstances, by no means mutually exclusive: (1) a latent dissimilatory trend, (2) a stimulating semantic or imagerial bridge, (3) false regression, i.e. a typical socio-linguistic reaction. Each single ingredient isolable from this amalgam can be charged with the full burden of responsibility for the crystallization of *plegaria*, but it is the joint momentum that most satisfactorily explains the outcome.

There is, needless to say, no reason to assume that situations of this kind are idiosyncratic to Romance. To supply a concluding illustration from Germanic, let me refer you to one set of idiomatic formulas in which that language family abounds: the irreversible binomials. An inquiry into these inlayings, which lend such verve and raciness to English and German, has disclosed that, in spontaneous and stylized discourse alike, several discrete forces are at work designing the shape of the binomials \((A + B)\). Chief among these forces, in modern Indo-European, are (a) one rhythmic pattern favoring the attachment of a longer \(B\) to a shorter \(A\); (b) one semantic pattern giving sequential precedence to the positive over the negative quality—"positive" and "negative" being relativistically defined as a matter of consensus within the frame of the given culture. There are on record certain binomials in which \(A\) precedes \(B\) by virtue of form alone (greater brevity)—Eng. *each and every*, *root and branch*, *part and parcel*, Ger. *auf Biegen und Brechen*—or of meaning alone ("positive", "concrete", "palpable" feature first)—*bigger and better*, *hither and thither*, with equality or near-equality prevailing in regard to the companion trait. Admittedly, in a few isolated cases a binomial has crystallized even though its outward configuration or its inner structure was so unpromising that this deficiency could have thwarted its genesis. But in the overwhelming majority of the cases it was, of all circumstances, a lucky coincidence of rhythmically appealing form and semantically attractive message that, in the first place, accounts for the birth and rapid propagation of the formula: *all or nothing*, *assets and liabilities*, *rise and decline*.

To summarize: The prevalent notion of unicausality needs thorough revision much as did until recently the long-accepted belief in dominant monolingualism (or, for that matter, the earlier typological classification of languages on the basis of a single structural characteristic, however salient). By starting out with the expectation of plausible pluricausality, we shall do fuller justice to the intricacies of reality.

The problem of concurrent causes must be very sharply divorced from the equally intricate and far more sorely neglected issue of primary vs. secondary causation. Let us assume that—in an undisclosed locale—through an interplay of substratum influence (which operated through bilingualism) and structural streamlining (which removed the rough edges), a given phoneme of the parent language, say \(-g-\), had dropped out intervocalically in the north while surviving intact in the south. Between the neatly silhouetted dialects of the extreme north and those, equally trim, of the extreme south there stretches a major transitional zone, which exhibits an, at first, perplexingly erratic distribution of \(-g-\) under the stated condition. Not only do the subdialects of this zone increasingly favor the
retention of the controversial occlusive as they approach or adjoin the 'solid south', but there emerge, again and again, in the same localities, words behaving in a mutually contradictory manner, even though one would expect their comportment to be alike. At closer inspection it turns out that the results are not entirely motley, inasmuch as numerous minor patterns become visible amid the chaos. Thus, it can happen within the confines of this area that words containing three syllables (or more) lose their -g-, while those of fewer syllables retain the consonant. Again it is conceivable that the place of word stress acts as a differentiator: If -g- precedes the accented vowel, its chances of survival may be quite different from what they would be under reverse conditions. Some adjacent sound may seem to produce the split: Though in the general phonological structure of the given language it makes no difference whether a consonant is flanked by /a/ or /o/, the fate of -g- in the ill-delimited dialect zone under examination may hinge on whether it forms part of the sequence /ga/ or /go/. Outside the realm of strictly phonological conditioning, it is not impossible that the loss of -g-, all other circumstances being equal, occurs with impressive consistency in adjectives, but neither in nouns nor in verbs; or that, within the verbal paradigm, -g- disappears only from certain moods, tenses, numbers, or persons. Beyond the domain of grammatical categories there extends the vast, hazily circumscribed zone of meaning: From this zone, too, speakers seem, occasionally, to have taken the cue in accepting or discarding certain forms involving the locally unstable -g-. It is surely no matter of mere coincidence if in a close-knit series of meliorative or propitiatory words, the -g- has invariably asserted itself, while in a comparable series of pejorative words (all other conditions, once more, being equal), the -g- was doomed to extinction. Clearly, this short list of readily identifiable determinants of a bifurcation does not exhaust the possibilities: In languages where vowel quantity is distinctive, long vowels before, or after, or both before and after, the -g- may have produced a special effect not observable otherwise. Such languages as grant structural significance to lexical tone may, in turn, impose a unique set of tonal conditions on an endangered consonant if that consonant is to emerge unscathed. Not only can examples of separate conditioning factors be almost indefinitely multiplied, with the understanding that only a selection of such factors is likely to be operative in any given language during a certain evolutionary phase, but characteristic combinations of these factors, productive of mutual reinforcement or reciprocal blocking, still further increase the range of possibilities.

In the older historical grammars the dividing line between all these kaleidoscopic varieties of minor causation and the necessarily few forces of major causation, such as external pressure and internal tightening, was not always drawn with all desirable precision. At this point, an attempt at clearcut hierarchization of these two categories must be made. If dialects sharply profiled in general, or at least conspicuously tidy in their treatment of certain sound changes, exhibit a neat cleavage, as when, in our imaginary country, the extreme north abandons the -g- which the extreme south faithfully preserves, then the chances are that a contrast of this magnitude, easily involving hundreds of examples, has been produced by the agency of a primary cause. But if, in a transitional zone
rich in blurred contours, minute coincidences in features of word stress, phonotactics, grammatical category, semantics, etc. must be painstakingly ferreted out to justify the self-contradictory comportment of exiguous clusters of lexical items (not infrequently as few as two), then it would be quite unrealistic to contend that these weakly represented determining conditions, which shall be known as secondary factors of causation, are on a par with, say, substratum influence or structural adjustment.

The underlying historical process is likely to have taken, typically, this course. For social reasons not directly related to the fabric of language, the cohesion of speakers in the north and in the south appears to have been such as to have produced two powerful nuclei of speech, each almost homogeneous within its own boundaries, though not identical with the other, witness—possibly among other conflicting peculiarities—the contrastive treatment of ancestral -g-. In the center of the territory—perhaps a kind of no man’s land for many critically important years—there arose no comparably leveled major dialect. Rather, as a result of successive infiltrations, annexations, partitions, and other disturbing changes of authority, there eventually crystallized a maze of subtly diversified subdialects, which only at a distinctly later date, through a delayed change in fortune for the raiders and settlers concerned, coalesced into a superficially unified dialect abounding in deep-seated discrepancies. Inside such a hastily assembled structure there must initially have co-existed countless doublets (e.g. pega ~ peA, tigur ~ tiur, etc.). In the course of the ensuing coalescence, the critical delay of the rapprochement and the continued lack of organic unity prevented, in each hamlet, the complete subordination of local autonomy to a territorial norm, i.e. either the sweeping abandonment of the -g- or its consistent restoration. On the other hand, superimposed over-all tightening was sufficiently strong to squeeze out any pointless duplication of forms, making it impossible for pega and peA, tigur and tiur to survive side by side for an indefinite length of time. As a result, there began to form small kernels of resistance to the spreading erosion of -g-, the leader words of the opposition being conceivably, at the outset, such lexical items as enjoyed greater currency in territories closer to the conservative south. On the analogy of these key-words, in whose ranks the process of selection was controlled partly by straight linguistic, partly by extraneous factors, speakers selected variants either with or without -g- of such other words as shared with the key words some characteristic feature of form (phonic, grammatical) or meaning. Practically any isolable detail of prosodic, phonemic, inflectional, derivational, compositional, or semantic structure could qualify for this role of secondary determinant, and it would obviously be quite misleading to regard the limited and delayed agency of such a random detail as a direct force of causation remotely comparable in its impact to potent substratum influence via bilingualism or to structural adjustment in the direction of economy and symmetry.

The situation is not radically different where, through a concatenation of historical antecedents and relative laxity of structure at the critical moment, a speech community has at its free disposal a number of competing derivational suffixes serving to produce abstractions from nouns, adjectives, and verbs, i.e. the
equivalents of Eng. -dom, -hood; -th, -ness, -ity; -ing, -al, -ment. Ordinarily, the suffixes in question are traceable to different sources or 'strains'; in English, e.g., one can readily set off those of Germanic, Anglo-Norman, and learned (Greek or Latin) provenience, and at stages of relative stability of usage it would not occur to speakers to detach such suffixes from the primitives to which they have traditionally been welded and to experiment with their free interchange. Thus, in contemporary English, it is inconceivable that anybody should toy, except in jest, with the substitution of -ity for -ness in coolness, deafness, eagerness, or, contrariwise, should try to foist -ness on the kernels of facility, stupidity (even if facileness and stupidness eke out a marginal existence in the most otiosely inclusive of our dictionaries), though there admittedly exists a thin fringe of free or almost free variation (briefness beside brevity, scrupulousness alongside scrupulousity). Now assume that in some unidentified language a group of such functionally related suffixes have been yanked loose from their moorings to specific derivatives and are thus freely available to enter into new relationships with an unlimited number of primitives. Make the additional independent assumption that for some undisclosed reason—narrowly linguistic or broadly socio-cultural—the tone-setting members of that speech-community are not, at this crucial juncture, economy-minded, i.e. are disinclined to seize this golden opportunity to effect the drastic reduction of the various 'morphs' of a single 'morpheme' to just one 'morph' (the equivalent of -hood, -th, -al, say). In the reshuffling of lexical and suffixal units that this mood of permissiveness is bound to entail, certain nuclei will predictably crystallize around chosen key words. If, for instance, the local counterparts of -al and -ment are each with striking frequency found attached to a conspicuous primitive, then these particular derivatives may well act as magnets in the ensuing reapportionment, attracting to the separate folds of -al and -ment, respectively, additional primitives sharing with the key words some such isolable feature as the number and configuration of syllables, the stress pattern, the grammatical form class, the semantic spectrum, etc.—regardless of etymological circumstances, of which the present generation of naive, aggressive speakers is, of course, utterly unaware. In a broad perspective it would be quite inaccurate to attribute to these secondary determinants, for all their importance, any major power of causation; they merely represent so many thin threads tying, in moments of cataclysmic flux, countless lexical items set loose by the flood to a few islets of relative security, i.e. of resistance to the unleashed forces of change. Primary factors of causation thus include the actual disruptive forces in linguistic growth; in contrast, secondary determinants, being analogical by nature, account for partial stabilization.

These very sketchy—and contrastively styled—illustrations of problems in the causation of language change do not begin to exhaust the range of legitimate inquiries into this domain, which is at the very heart of genetic linguistics. It remains to be seen whether the principles of linguistic etiology lend themselves to encoding in a rigid sequence of statements or formulas, or whether, on balance, a looser, more discursive presentation, of the type favored in historical research, is to be preferred as more germane to the analysis of events, in recognition of their infinite complexity.