Linguistics then and now: The view from NELS*

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1. NELS gets started

To facilitate interaction among students and researchers after the rapid growth of science in the late fifties and sixties, national scientific societies began sponsoring affiliated regional associations that organized regular conferences with published proceedings. The LSA, however, stuck to its traditional centralized model of two annual national meetings and a single journal. Younger linguists responded to the call for more flexible venues and publication outlets by starting independent regional linguistics organizations headquartered in major linguistics departments: the Chicago Linguistic Society in 1965, the Berkeley Linguistic Society in 1975.[1] Run mainly by students, they have continued to organize refereed annual conferences and to edit and publish the proceedings of them.

When Katya Chvany, Robert Channon, and I met in the summer of 1970 at the Linguistic Institute in Columbus and decided to launch a New England Linguistic Society to convene a conference at MIT that fall, we decided to address the problem in a somewhat different way. Instead of a localized organization like the CLS and BLS, we thought of a distributed community of linguistics departments, with every meeting passing the baton to the host of the next one. The slightly snarky wording of our announcement in the 1970 LSA Bulletin made a point of this lack of executive structure:

> The NELS, founded by a group of New Englanders attending the 1970 LSA Summer Meeting, is not an organization but rather an informal grouping to encourage communication among linguists in the New England area. There

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[1] Another response to the LSA's indolence was the Linguist List, also student-powered, which effectively replaced the LSA's Bulletin, and has operated since 1990 funded by donations from the linguistics community.

are no officers and no dues; any interested person in the New England area who wishes to be is automatically a member. One or more meetings a year will be held at different schools throughout the area; the program committee will include members from more than one school.

Our successful first NELS conference at MIT was attended by an unexpected number of Canadian participants, among them David Lightfoot, who offered to host the next conference at McGill University. The New England Linguistic Society gratefully accepted his invitation, and, adroitly salvaging its acronym, renamed itself the North East Linguistic Society to reflect its suddenly enlarged territory.[2] NELS has become very much a Canadian-U.S. joint venture, having met eleven times so far at five different universities in Canada, and returning there in November 6-8, 2020 to inaugurate its second half-century at UQAM. As one of the premier theoretical linguistics conferences, NELS now attracts participants from the entire world.

The obvious advantage of the floating conference model is that the regular change of venue ensures intellectual diversity and spreads the cost, effort, and benefits between different departments. It has been adopted by newer regional conferences such as WCCFL in the West Coast and GLOW in Europe, and later also by subfield conferences such as DIGS, SALT, SuB, and AMP. Like NELS, these conferences have grown steadily in size and stature, with prestigious proceedings that are often a step ahead of the big journals in reporting new ideas.

My story begins with a brief summary chronicle of NELS, relying mostly on the published volumes. I then take a closer look at NELS 1 and NELS 2, which did not result in proceedings, drawing on personal memories, on interviews with participants and organizers, and on a selection of talks that were published separately. In retrospect, two characteristics of those early meetings stand out: the diversity of the topics covered, and how little there was yet to build on. Many of the talks were the first treatments of issues that remain current today, and have still not been entirely superseded, though the questions they raised can now be approached in a deeper and more informed way, and more principled answers can be given to them. I conclude with a perspective on the past half century of linguistics viewed through the lens of NELS, with some reflections on the present state of the field, on the relationship between its subfields, and on their changing goals and norms.

2.   Timeline

NELS 1, MIT 1970. 2 days, 24 talks, selected by 3 organizers from 39 submissions. About 100-120 people attend the conference in Kresge Little Theater.

NELS 2, McGill 1971. More participants and an even more varied program than NELS 1.[3]

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[2]Sometimes referred to as the North Eastern Linguistic Society, and occasionally by hysteresis as the New England Linguistic Society. ("Linguistics" Society also occurs.)

[3]No program survives, but here are some of the presentations, with a reference to their eventual place of publication where available: Howard Lasnik, "Metrics and morphophonemics in early English verse" (Lasnik

NELS 3, UMass Amherst 1972. NELS is now established as an annual event, run by graduate students. UMass will host it again, for a total of five times.

NELS 4, Brown University 1973.

NELS 5, Harvard 1974. First printed proceedings.

NELS 6, UQAM Montreal 1975. Four papers in French!
Montreal Working Papers in Linguistics no. 6.

NELS V

NELS 7, MIT 1976. 200 copies printed. NELS will return to MIT six times (once sharing it with Harvard).

NELS 8, UMass Amherst 1977. UMass GLSA starts publishing the NELS proceedings.

NELS 9, CUNY 1978. First two-volume proceedings! Session on discourse.

NELS 10, Ottawa 1979. Notable talks: Higginbotham 1980, Huang 1980.

NELS 11, Cornell 1980. Lexicalist morphology reigns: Hasegawa 1981, Jensen 1980, Ladd 1981, Lapointe 1981, Muysken 1981.

NELS 12, MIT 1981. Parasession on word order. Agenda-setting papers Hale 1982 and Halle & Vergnaud 1982.

ALNE 13/NELS 13, UQAM Montreal 1982. Binding theory dominates the syntax sessions, and will do so for the next several years.

NELS 14, UMass Amherst 1983. As always, UMass attracts a strong contingent of semanticists, with talks by Chierchia, Rooth, Heim, and Horn.

NELS 15, Brown 1984. Influential papers on syntactic parsing and processing by Berwick & Weinberg, Carin & Fodor, Tannenhaus & Carlson.

NELS 16, McGill 1985. Discussion of parsing and acquisition continues.

NELS 17, MIT 1986. Barriers become a hot topic. Parasession on unaccusative verbs.

NELS 18, Toronto 1987. Phonologists focus on segments, features, and underspecification.

NELS 19, Cornell 1988. Null and quirky subjects are prominent on the agenda.

1990), David Lightfoot, "The nature of the subjunctive and optative" (Lightfoot 1975), Emily [Norwood] Pope, "Presuppositions conveyed by intonation" (Pope 1972), Carlos Quicoli, "On Portuguese impersonal verbs" (Quicoli 1976), Gary Miller, Problems in Greek accentuation (Miller 1976), Lyn Kypriotaki, "Variable rules and rapid English speech" (Kypriotaki 1973), R. M. R. Hall & Beatrice Hall, "Underlying VSO order in Indo-European", Margaret T. Stong, "On the concept *adjacency* in phonological theory" (Jensen & Stong 1973), Peter Binkert "On the insertion of *because*". Tony Kroch gave a talk on the relation between synchronic rule ordering and the relative chronology of sound changes (title unknown).

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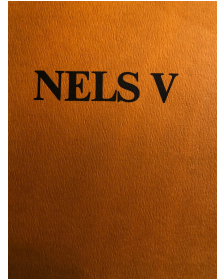
NELS 20, CMU/Pittsburgh 1989. Halle launches Distributed Morphology.

NELS 21, UQAM 1990. Classic paper by Clements unifies vowel and consonant place features.

NELS 22, Delaware 1991. Travis introduces Inner Aspect, von Fintel begins his voyage into exceptives.

NELS 23, Ottawa 1992. Ramchand presents the Aspect Phrase, Watanabe presents a novel analysis of factive complements.

NELS 24, UMass Amherst 1993. McCarthy and Prince introduce Optimality Theory.

NELS 25, UPenn 1994. Workshops on language acquisition and on language change.

NELS 26, Harvard/MIT 1995. 32 papers published in proceedings, 147 reviewers. Noam Chomsky gives plenary lecture. Sentence processing workshop, Indo-European workshop.

NELS 27, McGill 1996. Last NELS with no poster, parallel, plenary, or special sessions.

NELS 28, Toronto 1997. Poster sessions on syntax and phonology, printed in a separate volume.

NELS 29, Delaware 1998. Parallel sessions, two poster sessions.

NELS 30, Rutgers 1999. Optimality Theory, including OT syntax, is strongly represented.

NELS 31, Georgetown 2000. Roundtable on anti-symmetry and minimalism in Romance syntax. Free accommodations to students.

NELS 32, CUNY/NYU 2001. Special session on prosodic phrasing and parsing.

NELS 33, MIT 2002. Special session on non-configurationality.

NELS 34, Stony Brook 2003. Distribution of NELS proceedings turned over to Amazon.

NELS 35, UConn 2004. Special session on sign languages.

NELS 36, UMass Amherst 2005. Special session on "Topics at the Morphology-Phonology Interface".

NELS 37, Urbana-Champaign 2006. Westward ho! Special sessions on pidgins and creoles, and on psycholinguistics.

NELS 38, Ottawa 2007. Special phonology and semantics workshops, poster sessions. For the first time, semantics talks outnumber phonology talks.

NELS 39, Cornell 2008. Parallel sessions, two poster sessions, and a special session on Linguistics at the Interfaces.

NELS 40, MIT 2009. Parallel sessions, plus two workshops: Phonological Similarity, and Pronoun Semantics.

NELS 41, UPenn 2010. Parallel sessions, panel session on Unity of Linguistic Methods. Keynote addresses by Chomsky and Labov.

NELS 42, Toronto 2011. Elan Dresher and Will Oxford introduce contrastive feature theory.

NELS 43, CUNY 2012. East Asian Colloquium and Computational Linguistics Workshops.

NELS 44, UConn 2013. No parallel sessions! Special session on Locality.

NELS 45, MIT 2014. NELS goes three volumes!

NELS 46, Concordia University 2015. 98 presentations (including posters and alternates), selected from 382 submissions.

NELS 47, UMass Amherst 2016. Special sessions: Grammatical Illusions, Linearization of Syntactic Structures.

NELS 48, Reykjavík 2017. "Still on the North American tectonic plate..." Special session on Case Typology and on Locality in Phonology.

NELS 49, Cornell 2018. 112 talks and posters, based on novel data from about 50 languages, most of them understudied non-Indo-European languages.

NELS 50, MIT 2019. 3 days, 210+ registered participants, 94 talks and posters, selected by 461 reviewers from 430 submissions. Nearly four times as many talks as there were at NELS 1, yet with a much lower acceptance rate. Note the huge number of reviewers.

3.   The beginnings: NELS 1 (MIT 1970) and NELS 2 (McGill 1971)

NELS 1-4 remained unpublished. My archival and internet search for the actual programs drew a blank, and I was able to retrieve handouts of only a few of the talks. Here I describe some of the outstanding presentations from NELS 1 and NELS 2 (I did not attend NELS 3 and NELS 4) on the basis of their later published versions, with such context as I have been able to piece together by ransacking my memory and that of the other participants I interviewed. Although our joint trips down memory lane occasionaly ended up in amnesia alley, I am confident that what follows is reasonably accurate.

One of the highlights of NELS 1 was Ken Hale's "Relative clauses in some non-Indo-European languages", which appeared as Hale 1976. It drew attention to adjoined relative clauses, which had theretofore been overlooked in generative work, and had a huge influence that extended beyond syntactic theory and typology (Andrews 1971b, 1975, Lipták 2009) to semantics (Srivastav 1991, Dayal 1996a, Dayal 1996b, Bhatt 2003) and comparative Indo-European grammar (Kiparsky 1994, Probert 2015).

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Haj Ross' talk "Conjunctive and disjunctive questions", published as Ross 1973, pointed out an unsolved problem—a useful type of contribution that has regrettably gone out of style. It involves the following contrast:

(1)   a. Who remembers where John bought which books? (ambiguous)
       b. Who wonders where John bought which books? (unambiguous)

It elicited an immediate response by Kimball (1971), which never appeared in print because of the young author's tragic death, and is now lost. The puzzle was then picked up by Karttunen (1977), and again by Roelofsen (2019) in the framework of Inquisitive Semantics.

Avery Andrews' talk on predication (published as Andrews 1971a) was the first articulation of the idea that agreement builds feature-sharing structures, rather than just checking or copying features. It was later developed in GPSG/HPSG by Gazdar et al. (1985) and Pollard & Sag (1994), and adopted in different ways by Frampton & Gutmann (2000), Pesetsky & Torrego (2007), and Preminger (2017).

The most spectacular presentation at NELS 1 was Robert Rothstein's "Sex, gender and the Russian Revolution", printed in the Halle Festschrift (Rothstein 1973). Rothstein (p.c.) describes the audience's reaction exactly as I remember it:

> I began by saying, "Fifty-three years ago today, on November 7, 1917, a salvo from the cruiser 'Aurora' in Petrograd harbor marked the beginning of the Bolshevik Revolution." I didn't get to continue with the words "an event which, among other things, altered the surface structure of Russian sentences," because the crowd in Kresge Auditorium erupted with what the Soviet press used to describe as "stormy applause" (бурные аплодисменты) and "excitement in the hall" (оживление в зале)."

Rothstein's fascinating observations on Russian gender agreement and disagreement, with its pre- and post-revolutionary variations, have garnered much attention among sociolinguists.

(1)   Moja sestra moskvič-ka   / #moskvič.
       my   sister Muscovite-FEM / Muscovite
       'My sister is a Muscovite.'

(2)   Staryi vrač   ušёl   / Staryi vrač   ušla   / ᵀStaraja vrač   ušla
       old-M doctor left-M / old-M doctor left-F / old-F   doctor left-F
       'The old doctor left.'

Bobaljik & Zocca (2011) argued that infelicitous cases like (1) are actually not agreement failures, but violations of *Maximize Presupposition*: the feminine introduces the presupposition that the referent is female, and the masculine does not, so the more specific feminine form should have been used since it was possible. But as far as I am aware the pattern of variation seen in (2) remains unexplained.

Of the talks presented at NELS 1, the one that became most famous was Philip Lieberman's "On the evolution of human language", which likewise appeared in the Halle Festschrift (Lieberman 1973). Lieberman had reconstructed Neanderthals' larynxes and pharynxes, and found that they resembled those of human neonates rather more than those of human adults.

(3)

A	B	C
NEWBORN	NEANDERTHAL	ADULT MAN
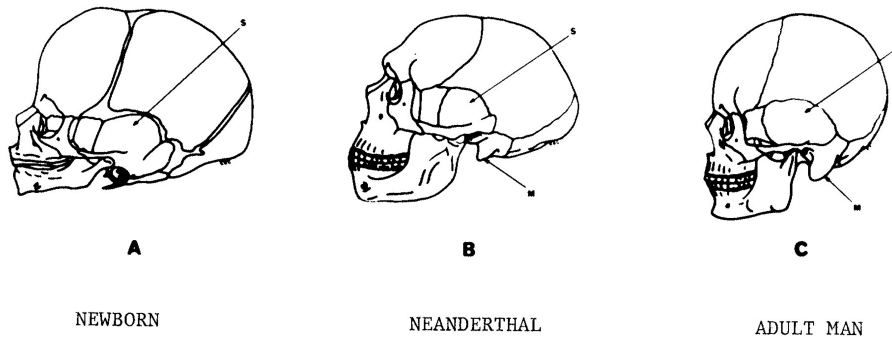
He argued that this vocal tract restricted the Neanderthals' sound production, making them unable to articulate the quantal vowels [i] [u] [ɑ] and velar consonants.[4]

Finally, Kroch and Lasnik's paper "A note on negatives: the *NOT*-Hopping problem" remained unpublished, but the handout, available on Lasnik's website, gives the gist of it.[5] It draws attention to a puzzling scope ambiguity between negation and certain modals.

(4)   a. John may not go out to play (without asking Mary first).
       b. (If he is tired), John may not finish the job until tomorrow.

Like Ross' talk, it sets out an unsolved problem—the scope of negation and modals. It was not addressed again head-on until Iatridou & Zeijlstra (2013) proposed their elegant solution: deontic modals obligatorily scope under negation, except where this would violate a PPI-licensing requirement, which is the case for PPI modals.[6]

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[4]Lieberman did *not* claim that Neanderthals could not speak. On the contrary, he surmised that they had the neural capacity for speech, just not the full articulatory repertoire of humans. He actually coached John Lone in speaking like a Neanderthal in the production of Fred Schepisi's 1984 movie *Iceman* (https://www.imdb.com/title/tt0087452/fullcredits).

[5]http://ling.umd.edu/~lasnik/Handouts-Conf%20and%20colloq/Invited.Keynote.Conference. That is what handouts in those days tended to look like. Although photocopying had been available since the sixties, it was still expensive, and handouts were often prepared with a machine that made ditto stencils off a xeroxed copy, each one good for cranking out about 60 purple-print pages.

[6]Among the other talks at NELS 1 were the following: Will Leben, "On the linguistic status of focus and presupposition", Richard Demers, "Rule insertion in Alemannic", Robert Faraci, "*And* as a verb-phrase complementizer", and Catherine Chvany, "Nominative alternating with genitive under negation". Since they were to my knowledge not published, I will not try to comment on them here. I return to my own talk at NELS 1 (published as Kiparsky 1972) directly below.

The talk that stood out at NELS 2 was Dorothy Siegel's "Expletive infixes and where you can shove them", later incorporated in her dissertation (Siegel 1974).[7] Since then, expletive infixation has provided not only entertaining material for phonology classes, but an enduring puzzle for prosodic phonology, which remains unsolved in spite of the efforts of eminent researchers like McCawley (1978), McCarthy (1982), and Yu (2003), among many others. McCarthy floated the conceptually attractive proposal that the foot structure of the host is minimally restructured to accommodate the infix. It explains, for example, why *un-F-believable* is OK but **ir-F-responsible* is impossible, for the foot structure is un(be((lieva)ble)), but (irre)((sponsi)ble), and the foot *irre-* can't be broken up. However, contra McCarthy, *unbe-F-lievable* is also OK—indeed, it is preferred to *un-F-believable* (at least it has many more Google hits).

Fortuitously, Tina Bögel's talk at NELS 50 may point to a way forward here. If prosodic phrasing can override lexical phrasing by prosodic restructuring, as she argued, we have an explanation for the acceptability of *unbe-F-lievable*, which has a canonical two-foot prosodic structure. There still remains the pattern seen with medial lapses in (5a), and the problem that it differs from the pattern (5b) reported for Canadian French by Baronian & Tremblay (2017).

(5)   a.   *?un-F-necessarily*, **?unne-F-cessarily* **?unnece-F-sarily*
      b.   *inter-F-minable* (Canadian French)

4.   Persistent and new themes

Phonology in the 70s faced a paradox: Chomsky & Halle (1968) (SPE) presented an attractive theory, which however produced, among many insights, also some unconvincing results, such as the supposed inaudible ε-glide suffix in *residence*, the phoneme /x/ in *nightingale*, and the phoneme /œ̄/ in *boy* /bœ̄/. In retrospect, attempts to rescue the theory by tacking on added conditions (such as my Alternation Condition, Kiparsky 1973) are just bad science: you can't fix a flawed theory by outlawing its wrong consequences. Arguing that you can get the Alternation Condition from a learning theory begs the question, for SPE theory is supposed to *be* an abstract learning theory. Rejecting SPE phonology in favor of Natural Phonology (Stampe 1972), still less for Natural Generative Grammar (Hooper 1976, Hudson 1980) does not resolve the issue since it kills many insightful analyses of SPE theory along with the bad ones.

The more productive line of response to the problem, well reflected in the NELS volumes, was to rebuild phonology so that it delivers reasonable analyses and a sound typology in a principled way. This research program lead to a new look that includes autosegmental phonology, metrical phonology, and later prosodic morphology and lexical phonology, and

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[7]Before getting to her subject she wondered whether it was OK to discuss such data in public. She was reassured by the fact that Quang Phúc Đông of the South Hanoi Institute of Technology, who was participating in NELS 2 as Jim McCawley, had recently begun to distribute dittoed articles with far filthier examples (sporting the stamp 𝔑ur für wiſſenſchaftliche 𝔷wecke that German libraries put on items in their *Giftschrank*). Dorothy Siegel (*in litt. el.* 6/12/2019) recalled: "For years afterwards, Jim McCawley asked if I was still interested in *fucking*."

introduces a type of rule that is restricted to derived environments. With these amendments, phonological theory no longer predicts obviously absurd analyses like the ones just cited.

The focus accordingly shifted from abstractness per se to exploring the role of phonology in language acquisition, perception, and production. One line of research, which took off in the mid-70's, aimed to test the accessibility of phonological derivations and underlying representations to language learners and users. Initially, evidence came mainly from language change, language acquisition, production experiments, and the manipulation of phonology in language games and metrical verse.

At NELS 8, Dresher (1979) demonstrated that certain puzzling Old English sound changes become intelligible when we recognize a system of phonological rules and derivations. Many similar cases have been reported in the literature on sound change (e.g., Kiparsky 2015, Bermúdez-Otero 2015).

In an elegant study, Myerson (1978) showed that children begin to overgeneralize vowel shift at age 11-14. Based on previous findings that rules tend to be overgeneralized when they are first learned, Myerson argued that vowel shift is acquired at this time, since the vocabulary that provides the bulk of the evidence for it is learned then. Another interereresting discovery that came from her experiments is that regularization in the direction predicted by phonological rules of the type we would now identify as lexical, such as vowel shift in English, is strongly manifested in long-term memory even when short-term recall or production tasks (wug-tests) fail to reveal it. This result suggests that the main function of lexical phonology is to serve the learning, recognition, and recall of vocabulary by enabling speakers to discover systematic phonological relationships between words.

Myerson's and Dresher's discoveries are two of many strands of evidence that converge on the conclusion that the output of lexical phonology has a special status and has the cognitive significance that was erroneously attributed to the structuralist phoneme. I have assembled the arguments at length in Kiparsky (2018). Some of the most compelling evidence comes from the manipulation of phonology in poetry and language games. In retrospect, it includes the studies presented by myself at NELS 1 (Kiparsky 1972) and by Lasnik at NELS 2 (Lasnik 1990). We argued that oral poets can compose verse on the basis of an intermediate phonological representation that they access on the basis of their implicit phonological knowledge. Since Lasnik's study is particularly compelling, I summarize it here.

Until about 950, Old English poetry obeyed, alongside the usual alliteration convention that a consonant alliterates with another identical consonant, the unexpected rule that [k] alliterates not only with [k] but also with [č], and that [g] alliterates not only with [g] but also with with [ǰ] and [j].[8]

(6)   a. oþþe þæs **c**easterhlides, **c**lustor onlucan [Christ 1: 314]
         or   of the city gate,   the   lock   open
         'or open the lock of the city gates'

      b. to ge**c**eosenne **c**yning ænigne [Beowulf 1851]
         to choose   king   any

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[8]In (6) the words *ceaster-, ceosenne,* and *gear-* are pronounced with [č] and [j].

'to choose any king'

c. þone on **g**eardagum **G**rendel nemdon   [Beowulf 1354]
   whom in days of yore Grendel they named

Traditional explanations see this alliteration pattern as an orthographically based conven-
tion. Lasnik convincingly refutes this idea by pointing out that the poetry is largely oral,
and moreover that when it it is written, the alliteration doesn't care about the spelling. [k]
can be written *c*, *k*, and sometimes even with the runic letter ᚴ, and they all alliterate with
each other.

(7)   a. woldon **c**eare cwiðan   ond **k**yning mænan [Beowulf 3171]
         would   sorrow tell,   and   bemoan the king

      b. ᚴ ᛗ ond ᛉ   **c**yning biþ   reþe [Juliana 704]
         cen ȳr and *nȳt*   the king will be harsh

Another traditional explanation for the alliteration pattern (6) is that lines composed prior
to palatalization are transmitted in frozen form. Lasnik disposes of this argument too by
noting that even original **j-*, which never was *g-* at any stage of the language, likewise
alliterates with *g*:

(8)   a. **g**eongum and ealdum,   swylc him **G**od sealde   [Beowulf 72]
         to young and old,   such as them God had given

      b. mid   **I**udeum   **g**umena wiste   [Elene 1202]
         among the Jews   of men she knew

Lasnik's own convincing analysis starts from the understanding that OE [j, ǰ] and [č] were
respectively derived from /g/ and /k/ by synchronic palatalization before front vowels. Allit-
eration operated in the phonological derivation preceding the operation of the palatalization
rule.

(9)   Infinitive   Past plural   Past participle
      ceosan [čeozan]   curon [kuron]   coren [koren]   'choose'
      gieldan [jieldan] guldon [guldon] golden [golden]   'to pay'

He goes on to motivate the reanalysis of the system in late OE by a series of changes that
made the previously transparent palatalization rule opaque. As a result of these changes,
velars and palatals could no longer alliterate with each other. He illustrates this by the
contrast that arose between strong and weak verbs:

(10) Strong   Weak
      brecan [k]   'break'   secan [č]   'look for'
      drincan [k]   'drink'   drencan [č]   'drench; cause to drink'
      springan [g] spring   sengan [ǰ]   singe
      scrincan [k] shrink   hnǣgan [j]   neigh

Under SPE phonological assumptions, Lasnik had to argue that [č] and [k], and [ǰ] and [g], simply became underlyingly distinct. But of course phonologically conditioned alternations like (9) continued unabated for some time after 950. This weak point of his analysis can be resolved by recognizing that phonologies are stratified. From the perspective of Lexical Phonology, what happens at this point in the language is that palatalization becomes restricted to the lexical component of the grammar, and alliteration between velars and palatals stops because the poetic conventions apply to the output of the lexical phonology.

A similar development seems to have caused the collapse of the Classical French versification canon that was in force ca. 1600-1900:

(11)   a. Consonants that are deleted in word-final position count for purposes of rhyme: *son* does not rhyme with *long tronc*, *rond*, or *pont*

           b. But homorganic final voiced and voiceless obstruents are treated as equivalent: *long* rhymes with *tronc*, *rond* rhymes with *pont*

In this system, rhyme can't be defined on phonetic and traditional phonemic representations (a), nor on underlying (morphophonological) representations (b). But they can be defined on the *lexical* representations (c), that is, on the output of the lexical phonology:

(12)   a. /tʁõ/ [tʁõ], /lõ/ [lõ], /mõ/ [mõ], /ʁõ/ [ʁõ], /sõ/ [sõ]

           b. {tronk}, {long}, {pont}, {rond}, {son}
               (cf. *tronquer, longue, ponter, ronde, sonner*).

           c. *lonk*, *tronk*, *dont*, *ront*

The consonant devoicing process also furnished the inputs to the classical *liaison* system that was contemporaneous with the rhyming convention.

(13) *long hiver* [lõ.ki.vɛʁ] 'long winter', *grand homme* [grã.tɔm] 'great man'.

In Brazilian Portuguese, invented language games seem to show that the four nonlow nasal vowels [ĩ, ẽ, õ, ũ] are derived, whereas the low nasal vowel [ɛ̃] is an underlying segment (Guimarães & Nevins 2013).

(14)   a. Stems in {-an-} regularly contract with a following ending {-a}, e.g., *sã* [sɛ̃] 'sane' (fem.), from /san-a/ (cf. masc. *são* [sɛ̃w̃], from /san-u/).

           b. {-Vn-}stems where V is some other vowel than /a/ keep the stem form under these circumstances, e.g., *dona* [ˈdonɐ] 'lady', from /don-a/ (masc. *don* [dõ], from /don-u/), or in exceptional cases delete the nasal, e.g., *boa* [boɐ] 'good' /bon-a/ (masc. *bom* [bõ] /bon-u/).

This distribution falls out if the low nasal vowel is formed at the stem level, whereas the other nasal vowels are formed postlexically.

(15)   a. {san-} → /sẽ/ → /sẽ-a/ → [sẽ]
        b. {don-} → /don-a/ → [ˈdonɐ]

For other similar cases see Malone (1988), Malone (1862), and Zeps (1963); for other kinds of evidence that the output of lexical phonology is a "psychologically real" level of representation, with discussion of the similarities and differences between that level of representation and structuralist phonemics, see Kiparsky (2018).

5.   Reflections on the present

The following graph shows my count of the average number of talks (including posters) in syntax, semantics, phonology, and morphology per meeting in each of the first five decades of NELS.

NELS presentations by topic
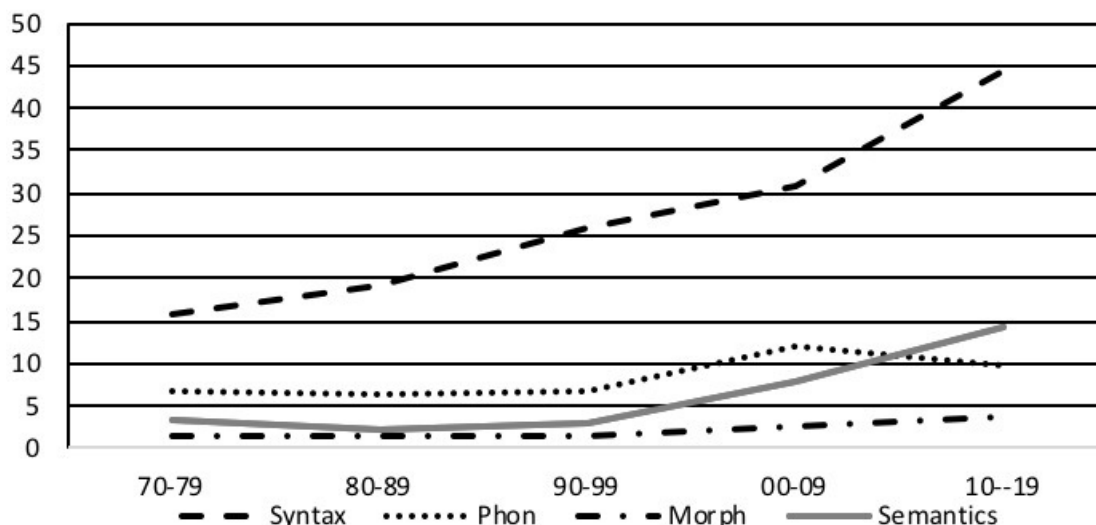
The graph raises two immediate questions. First, where is historical linguistics? Why does it have such a limited presence throughout the period, with the exception of the special workshops devoted to it at NELS 25 and NELS 26?[9] And secondly, why has the representation of phonology and morphology remained more or less flat, and in proportional terms even decreased?

    The underrepresentation of language change as a topic on the agenda of NELS and other conferences devoted to formal linguistics is difficult to explain. Surely it can't be justified by Saussure's now discredited claim that synchrony and diachrony require two entirely different *kinds* of linguistics. Possibly it somehow reflects a lingering notion that historical linguistics is merely descriptive or "applied" and has no theoretical content of

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[9]I am concerned with work that directly addresses issues of language change. Not as scarce are synchronic analyses based on data from earlier stages of languages, such as Rothstein's talk at NELS 1 and and Lasnik's talk at NELS 2, both summarized above.

its own. A glance at the programs and proceedings of DIGS, conveniently gathered at http://walkden.space/digs/, should dispel such superstitions, and shows that in reality it is, if anything, *more* theoretical, because to do it right you need all of formal linguistics and then some. My best guess is that the quarantining of diachronic linguistics has simply become an arbitrary convention of the field, entrenched in graduate curricula and departmental structures, without any synchronic justification, but historically rooted in Saussure's views of a hundred years ago, whose persistent effects even Jakobson's and Halle's efforts were unable to remove.

As for the relative decline of phonology and morphology, it is a more recent development with a more transparent etiology. It is not that these subjects are somehow less interesting or less important than the other subfields. In fact, they have been every bit as fertile in generating new theoretical ideas during these 50 years as syntax and semantics have been. Phonology in particular retains its traditional role as a pilot branch, the natural vanguard of linguistics, simply because it is a relatively manageable domain in which new technical ideas can be tested first, and many incommensurable approaches are able to flourish.

Rather, phonology's intrinsic avantgardism makes it not only exciting, but also FRAG-MENTED, putting it at risk of being marginalized within the field. By fragmentation I mean the division into subsubfields and subsubsubfields that happens when specific analytic or methodological approaches set themselves off as so many new kinds of linguistics. We have formal phonology, Laboratory Phonology, and Corpus Phonology, the former further fractured into OT and rule-based versions, with each in turn split into classical, harmonic, stratal, and several kinds of probabilistic OT phonology, and into natural, government, and rule-based phonology. Different views about phonological representations are reified as metrical phonology, autosegmental phonology, particle phonology, and substance-free phonology.

The proliferation of approaches per se is not a problem but a sign of health—indeed, it is essential to any scientific endeavor. But not every new approach should spawn a new field. The various theories about phonological representations address different aspects of structure and are perfectly compatible with each other. They are also largely orthogonal with respect to the competing theories about the organization of phonology, and with respect to those about phonological processes. And the entire new methodological repertoire can in principle be used to explore any of the issues. None of the innovative ideas are in any case so massively transformative that they constitute a whole new branch of phonology.

Just as the proliferation of ideas and methods is healthy, so obviously is the proliferation of research communities devoted to their exploration. The problems come when they stop talking to each other. It is easier to just "do" an OT or SPE-type analysis of a set of data than to do both and assess their relative merits, but it is in the latter activity where most of the science lies. At least as much effort should go into theory comparison as into honing formalisms.

The same trends have affected morphology even more. Paradigm Function Morphology, Distributed Morphology, Minimalist Morphology, and Construction Morphology all address much the same theoretical issues and empirical phenomena. Dialogue between them should be easy and productive, and yet their advocates barely take notice of each

other in their writings. Morphology papers typically assume one of them and address some data in relation to what it says about how the theory should be articulated in detail. Shockingly, even morphology conferences tend to be specialized to a single approach. The recent International Morphology Meetings (Budapest 2018, Vienna 2020) have had barely any DM talks, and there have also been morphology conferences devoted almost entirely to DM.

Fragmentation also creates several practical problems. For one thing, what do we teach? In phonology, should we start with ordered rules and proceed to some version of OT? If so, should OT make up the main part of the course, or a few lectures at the end? Or should we do the reverse? Or perhaps teach them simultaneously exploring their merits as we go along? In morphology, do we start with naive lexicalism and then go on to DM, PFM, MM, or some other approach? Or plunge directly into one of the latter?

Another negative consequence was pointed out to me by Edward Flemming: conference abstracts submitted in a divided subfield will naturally tend to receive a share of harsher reviews from the partisans of rival approaches in those subfields, which will reduce their overall competitiveness relative to the abstracts in less sectarian subfields.

At a higher level, fragmentation of the field itself into self-contained subfields is perhaps to some extent an inevitable consequence of its growth. It is simply getting hard to keep up even with neighboring subfields. But history shows the benefits of maintaining contact between subfields. The rapprochement between syntax and semantics is one of the success stories vividly reflected in the volumes of NELS, and more recently syntax and morphology have also drawn closer, revealing exciting new perspectives. Everyone now recognizes that early generative phonology neglected morphology, much to its detriment, a rift that was fortunately repaired when morphology became formally developed and its subject matter got more clearly defined. But now the tables have turned to the point where morphological theories tend to neglect phonology, even though this time there is no excuse. For example, some recent morphological theories have ignored the fact that the distribution of morphemes and allomorphs can be phonologically conditioned. Taking phonology seriously allows a proper demarcation between phonology and allomorphy. This eliminates many redundancies and theoretically problematic non-local and outward allomorphy dependencies; for development of this point with examples from English morphology see Kiparsky (2020).

6.   Summing up

Reviewing the past half-century of NELS has been an eye-opening exercise. Surprisingly, almost all the issues addressed a half century ago are still alive. But today we can understand them more deeply and approach them in a broader context. A host of new questions have emerged, at a level of specificity that we could not even dream of then. And completely new ways of answering them have become available. Variation, in the old days dismissed as noise to be excluded from the data, is now recognized as a valuable source of information on linguistic structure, its systematic patterns reflecting language-specific and even universal constraints and preferences. Experimental methods, from their modest beginnings as probes for the "psychological reality" and mental representation of linguistic

constructs, are now routinely used for adjudicating between competing hypotheses in all branches of linguistics—most recently, and very successfully, in semantics.

In the final section I raised two questions: Why was language change never part of the show? And why has the representation of phonology and morphology not kept pace with that of the other subfields?

My answer to the first question was tentative and perhaps unconvincing. Regarding the second, I am more confident about the generalization that a subfield is successful in proportion to the extent that it is able to embrace diverse theoretical perspectives within a cohesive research community. This is harder to achieve in a complex and rapidly developing subfield. But while fragmentation is understandable, it is not inevitable, and once it is recognized as an obstacle to theoretical progress, it should be possible to overcome it with some good will and effort.

In any case, those of you who gather to celebrate the centenary of NELS in 2069 will surely be able to look back proudly at the next 50 years and have some great stories to tell about them.

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