Fermi National Accelerator Laboratory

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The Fermilab Upgrade*

Leon M. Lederman
Fermi National Accelerator Laboratory
P.O. Box 500, Batavia, Illinois

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I. Introduction

In 1978, Fermilab set out a goal of building a superconducting accelerator (Energy Saver) which would raise the proton energy to close to 1000 GeV for operation in two modes. Tevatron I would provide proton-antiproton collisions at a total CM energy of near 2.0 TeV to study the particle mass domain beyond 100 GeV. Tevatron II would provide extensive facilities for the programmatic study of Standard Model physics in an upgraded fixed-target program. There was of course the realization that with the right mixture of precision and imagination, the collider could add significantly to Standard Model physics (e.g. W and Z physics, W,Z pairs, B-physics) and that the fixed-target program could explore beyond the Standard Model (e.g., rare K-decays, CP violation). In 1988, we are engaged in setting out the future program of the Laboratory based upon the success of the Energy Saver, TeV I and TeV II construction programs. This future program assures that operation of the TEVATRON facility for physics is the overriding priority between now and perhaps 1993 and it also assumes that the Superconducting Super Collider (SSC) will be funded for construction in 1990 and will begin producing physics by 1999.

A "brief history" of upgrades is presented in section XI.

II. History

The notion of going to higher luminosity in the Collider and more intensity and quality for the fixed-target program has been around since

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* This is based upon the work of many people over a long period of time. In particular Steve Holmes and Estia Eichten have been most helpful.

the start of TeV I and TeV II. The simply stated goal in collider physics is to increase the mass range which can be searched for new phenomena and in the fixed-target program to enhance the precision and the detail of our Standard Model base. In Laboratory presentations we have proposed a Superbooster (1980), Dedicated 4 TeV Collider (1983), Brightness Enhancer (Jan. 84), and Source Brightener (Sept 84). Upgrade plans and funding profiles were presented in the 1986, 1987 and 1988 institutional plans. Responses from HEPAP have been positive going back to 1982.* Experience with the first engineering run of TeV I in 1985 and the 1986 construction year led to a thorough review of the entire accelerator complex. A Collider upgrade plan was submitted (short form 44) with a TPC of $267M in January 1986.

As the first phase, the Linac Upgrade was submitted in January 1987 and resubmitted in February 1988. The plan has emerged into two stages: an adiabatic series of improvements which will bring the peak luminosity of the $\bar{p}p$ collider to about 5×10^{30} cm^{-2} sec^{-1}. This should also make over 3×10^{13} ppp available to the fixed target, an improvement of almost a factor of two. The Collider energy would be 1.0 TeV and the fixed-target energy near 900 GeV. Given reasonable R&D funds and the Linac line item, all of this should be available for a D0 and CDF run of $\int Ldt > 10$ pb^{-1} in 1992.

In the period until 1993, there would be no planned shutdown in excess of several months for installation of upgraded components. This period would also see modest upgrades to Collider Detector at Fermilab (CDF) and some decisions on major new detectors and upgrades for the fixed-target program. In 1993, one can contemplate a 6-10 month shutdown for the second phase of the upgrade. This would be designed to deliver in excess of 100 pb^{-1} per run to the collider detectors and in excess of 4×10^{13} ppp for the fixed-target program. Given enough protons, it will pay to improve the fixed target duty cycle even more - perhaps from 30% to 60%.

There are now several competing elements for the second phase of the upgrade. The purpose of this note is to review these which, at this writing, are evolving out of extensive high-energy physics (HEP) community discussions.

III. Review of Upgrade Motivation

The Fermilab collider is the highest energy machine in the world. Until SSC or LHC or the Soviet 3 GeV x 3 GeV collider turn on and begin to produce physics data, this will remain so. We believe we have a time window that will go to 1999 or so since it will take several years for any of the above machines to go from commissioning to real physics. The window is not only an opportunistic window, it is essential that there be continuity in the production of physics results. Whereas, if SSC is proceeding towards, say, a completion date of 1997/8, a fairly large community will be occupied there by 1992, but one cannot put graduate students, new postdocs and pre-tenure professors on many of the SSC detectors until they are much closer to physics. This is borne out by CDF and D0 experience. The Fermilab Collider physics in the period 1994-1999 will also be invaluable as a guide to SSC both from the point of view of collider and detector technology but also from the physics knowledge base. Since a year of SSC is worth $250M (1988), it is terribly cost effective to be as well prepared for the SSC era as one can possibly be. Finally we note that there may well be niches of physics for which TEVATRON energy is well enough above threshold; a vast increase in energy may then only increase backgrounds.

The knowledge base will come from both the Fermilab Collider and the fixed-target program, especially those experiments which illuminate high-rate technology and those which use precision and detail to test and extend the Standard Model.

To present a glimpse of the relative merits of the various upgrade options we present a series of graphs calculated by E. Eichten. We stress that whereas the optimum plan is not yet clear, what is perfectly

clear is that the design goals are such as to double the discovery limits, i.e., equivalent to doubling the effective machine energy. Furthermore, it makes possible the collection of huge amounts of data for particles in the W, TOP, e.g., $\lesssim$ 125-GeV mass range.

A doubling of the mass reach could be compared to building a 400-GeV e^+e^- machine with sufficient luminosity to double the mass reach of LEP II. Another comparison scale is the current attention to B-physics and proposals for electron-position B-factories. An upgraded TEVATRON has impressive capabilities here although the issue is complicated by backgrounds.

The potential for discovery of new physics by our upgrade or for the clarification of discoveries which may be made in the early stage of TEVATRON are very significant. We also stress the important support this kind of data gives to SSC where the parameter $M/\sqrt{s}$ will very rarely reach the Upgrade goal of ~ 0.4.

Advancing fixed-target physics will be critically dependent upon advancing the art of detectors. Exploiting higher luminosity in the collider also requires confidence that the detectors are up to resolving signal and background in the high rate environment.

IV. Upgrade: Phase I Goal 5×10^{30} cm^{-2} sec^{-1} and 3×10^{13} ppp 1988 - 1993

The first phase involves a series of steps:

1. Replace Cockcroft-Walton by RFQ, new first tank on linac.
2. Replace last 5 linac tanks by side-coupled cavity type of tank at 800 Mhz (instead of 200 Mhz). This will raise the energy of protons injected into the Booster to 400 MeV. The transverse emittance should go to 12π mm.mr or even as low as 6π at 1×10^{10}p.
3. Strong low-β quadrupoles for D0, CDF; Goal $\beta^* = .25$m.

4. Possible shorter bunches.
5. $\bar{p}$ Source and cooling improvements.
6. Dipole magnet development for separator space; goal is 6.6T dipoles.
7. Cryogenic developments to achieve about 3.9^{o}K TEVATRON for 900-GeV fixed-target and 1000-GeV Collider operations.
8. Electrostatic separators - helical orbits. 50 kV/cm for 2.5cm gap.

These steps, carried out by AIP, R&D and Linac Line Item funding can and should be complete in time for a 1992 run of CDF and D0 with a goal of $\geq 10pb^{-1}$. Included here are already scheduled improvements in the CDF detector, completion of the D0 detector and new starts on a major fixed-target spectrometer, given PAC approval. Other fixed target activity involves continued upgrades of major existing detector facilities.

V. Upgrade Phase: II Goal $5x10^{31}cm^{-2}sec^{-1}$ and $4x10^{13}$ ppp at > 50% Duty Factor 1994-1999

Introduction

We have looked in some detail at several approaches to this next factor of ten. The luminosity goal is designed to keep the CDF and D0 detectors from melting, but this luminosity will require substantial upgrades to both detectors. These involve replacement of front-end electronics, perhaps central tracking and perhaps some calorimetry improvements. Consideration is also being given to a possible third collider detector, which would be specifically designed to do B-physics. What is also open is whether this gets its own collision region or goes in to alternate with CDF, say. Finally, considerable weight is given to the fixed-target program and how it is benefitted from the various options. Whereas the TEVATRON Collider mode may be supplanted by SSC, the fixed-target program will probably extend well into the SSC era, taking

advantage of SSC detector R&D, the almost certain need for more precision and detail, and the continuous need for test beams. We now list the options as currently understood and later indicate some variations and phasing possibilities.

A. $\bar{p}p$ with Superboosters

Here, in order to supply two IR's with $5x10^{31}$ luminosity we need an improved source fed by an improved Main Ring (MR) and a place to store $3x10^{12}$ $\bar{p}$'s. Some means of recovering $\bar{p}$'s which have diffused is also useful.

The major devices here are two 20-GeV rings; one, the proton superbooster, injects into the Main Ring at 20 GeV yielding high transmission, small emittance ($\lesssim 12\pi$), good lifetime and high proton intensity for proton production. The second ring, a $\bar{p}$ ring, is an antiproton depository. This would also involve 8-16 GHz cooling in the $\bar{p}$ accumulator and depository. The total cost including R&D, pre-op is $124M. The technical problems of actually achieving $5x10^{31}$ are formidable. A more conservative goal is to have a 5-month run (repeated annually) to yield an integrated luminosity of 100 pb^{-1}.

B. pp Option

This suggests a pp option, where more than $5x10^{31}$ is assured and an overall efficiency of twice the $\bar{p}p$ option seems reasonable. We then assume that we can collect 500 pb^{-1} in a collider portion of one year run. Higher luminosity, i.e., $2x10^{32}$, can be achieved for special purposes not including the normal operation of the CDF and D0 detectors. Another virtue of pp is the small interaction diamond which benefits all short lifetime experiments, e.g., B-physics. The pp option, as an accelerator project, is not particularly challenging. However, it requires removing the MR from the tunnel (it becomes the Main Injector) and providing a 120- to 150-GeV tunnel into which MR components would go. All overpasses and other TeV-MR hindrances would disappear. The

new injector could also be a $\bar{p}$ producer and be organized to provide 150-GeV test beams during Collider operation. MR removal would allow space for a second superconducting magnet string. Longer straight sections would be needed in order to bring beams into collision. This could be done with small displacements of the CDF and D0 detectors. The total cost estimate here is about $240M.

C. $\bar{p}p$ High Energy Option

A third option removes the MR and/or the TEVATRON from the old tunnel and replaces it with a ring of 6.6 to 8T superconducting magnets of the SSC/HERA style. This would permit $\bar{p}p$ operation at 3 - 3.5 TeV in the CM. Since there would be no superboosters, the luminosity would be only slightly better than $5x10^{30}$ that was achieved in Phase I. Both CDF and D0 detectors would work well here with much less extensive upgrades than option B. The mass reach of such a 3.5 TeV $\bar{p}p$ collider is about that of a 2.0-TeV collider at $> 5x10^{31}$. The fixed-target program can gain substantially from a higher energy extracted beam and/or the higher intensity of secondary and tertiary beams. The improvement factors come from the benefits of a redesigned Main Ring (Main Injector) and the luminosity gain due to the higher energy (1.5 - 1.8 TeV). We take 10^{31} as the design luminosity and therefore an integrated luminosity of 30 pb^{-1} per year. The energy increase could be a significant help in many fields, e.g., in heavy quark studies, in hyperon research and in structure function data. Open questions here have to do with the removal of the MR and the cost of higher field magnets. This will probably come to about the cost of the pp option.

VI. Selection Criteria

Which of the three options (or none of them) to choose will depend on a number of criteria:

(1) Physics reach in the collider mass domain "beyond the W, Z"

(2) Implication for advancing fixed target physics
(3) Cost
(4) Time and downtime to implement
(5) Detector Implications
(6) Technology experience relevant to SSC
(7) B-physics
(8) SSC at Fermilab? If so, it may favor one option more than others.

Many of these criteria are not simple. Physics reach with high luminosity is clouded by backgrounds, pile-up, etc. It may be useful to assume the following about detectors:

1) CDF requires new electronics at $\geq 5 \times 10^{30}$ @ \$10M
2) D0 requires new electronics at 5×10^{31}
3) CDF will require new tracking, vertex, etc. at $\sim 10^{31}$
4) Both detectors will require much more major upgrades at $> 5\times10^{31}$. However, even these upgrades, at an estimated total cost of \$25M each, are much less in time, money and people than starting over.
5) With SSC demands, it makes no sense to contemplate a brand new "standard" 4π detector. New ideas, however ...

VII. Phased Options

As phase II in the upgrade one can consider building a new main ring of 120-150 GeV in its own tunnel with new magnets but, initially, only minimal power supplies. This could be constructed and commissioned without interference with the on-going program. Its objectives: i) excellent injector into TEVATRON; ii) excellent $\bar{p}$ producer e.g. 2 sec/cycle; iii) provides 150-GeV beams to fixed-target program during collider runs, saving - 2 months of calibration, timing, commissioning of fixed-target experiments; iv) may provide very intense neutrino and k-beams for special experiments. This would also free CDF

and D0 from Main Ring backgrounds and provide a space for another interaction hall at E-ZERO. Also, it frees space in the existing tunnel for another superconducting ring. When completed and commissioned, there would be a shutdown for tunnel connections, moving of MR power supplies, etc., and perhaps removing MR magnets. This may be ~ 3 months. Options B or C would follow as phase III.

Other phases, as demanded by physics and allowed by resources would be to upgrade from the modest luminosity of the 3 TeV option to perhaps 3-5x10^{31} using option A devices. Alternatively, if the original TEVATRON ring is still in the tunnel, pp collisions (1.5 TeV x 1.0 TeV) can be contemplated, especially for the B-detector but perhaps for additionally upgraded CDF/D0.

VIII Summary: Physics

Our options as of July 1988 are now recapitulated. We assume a 5 month collider run, 5 month fixed-target run and 2 months of changeover, studies, etc.

A. $p\bar{p}$ $\sqrt{s}$ = 2 TeV	$\mathscr{L}$ dt = 100 pb^{-1}/year
B. pp $\sqrt{s}$ = 2 TeV	$\mathscr{L}$ dt = 500 pb^{-1}/year
C. $\bar{p}p$ $\sqrt{s}$ $\geq$ 3 TeV	$\mathscr{L}$ dt = 30 pb^{-1}/year

The physics graphs and Table take into account the different quark content of pp and $\bar{p}p$.

From the graphs and from the Table, it is clear that the TEVATRON upgrade has two physics benefits. Any of the options extends the discovery potential for a characteristic subset of theoretical speculations by a factor of two in mass: it permits a thorough exploration of the interesting 200-400 GeV mass domain - "the foothills of the TeV summit." Recall that in new Technicolor theories, the crucial parameter is F_π = 246 GeV.

Equally significant, for masses near the lower end, it provides "factory" potential. TOP is an excellent illustration. If, as some theorists intimate, the TOP mass is under 125 GeV, then the upgrade makes tens of thousands of TOP quarks per year and thus defines a TOP factory. This applies to many of the potential discoveries - one will be able to exploit the discovery of a GLUINO or TECHNIPION in some detail if the masses are not too high. Perhaps all the theories are wrong - still the exercise indicates that whatever nature has in the 50-400 GeV mass domain, the TEVATRON upgrade will be a powerful tool to guide particle physics on the correct road from the Standard Model toward the ultimate unification.

We have not yet listed some of the obvious "goodies" that have been widely discussed elsewhere:

<u>b-quarks</u>: The upgrade will result in of the order of 10^{10} $B\bar{B}$ per year pairs with option B giving 10^{11} $B\bar{B}$'s. Fermilab proposal P-784 has under design a detector which can carry this to the observation of CP violation.

<u>W + Z's</u>: The 100 pb^{-1} luminosity yields 10^6 W's per year and $2x10^5$ Z^0's. With precise Z^0 masses derived from e^+e^- machines and a highly precise mass ratio of W to Z, one can derive unique values for important radiative corrections which involve the Higgs mass.

Compositeness, Drell-Yan, Fourth Generation and many other processes and issues will also be addressed.

<u>Fixed Target</u>: Although we have stressed the benefits to the Collider, the gains to the fixed target are also important with option C probably having the largest influence. Here even a modest increase in energy gives a very large increase in, for example, photoproduced B's (factor ~ 20). Secondary beams

gain in energy and intensity, hyperon beams also gain from the increase in laboratory lifetime.

IX. Funding Scenarios

In our firm, unalterable 15-year plans we have presented funding profiles which have not noticeably produced cardiac arrest among DOE readers. Table X (Profile I) out of the 1988 Institutional Plan is typical. Below this is an alternative plan which assumes less civil construction and more R&D in the realization of the upgrade program. It assumes we do something between the costs of $\bar{p}p$ and pp or $\bar{p}p$ at high energy. The difference is ± $10M/year. It includes funds for detector upgrades and fixed-target initiatives.

X. Constraints

In guiding this discussion we have in fact made a number of constraining assumptions:

1. The non-SSC funding level of $560M will not be increased during SSC construction.

2. SSC physics will be in full swing with first physics publications by ~ 1999.

3. The upgrade over the period 1989-1994 should require increments to the Fermilab budget of less than $50M/year.

4. No new 4π detector can be contemplated. CDF and D0 may be upgraded but not replaced. A special purpose new detector for B-physics is conceivable if its cost is modest compared to original CDF/D0 costs.

5. The upgrade should begin to produce physics by 1994-5.

6. Until 1993 we plan no shutdowns in excessive of 6 weeks.

7. CDF and D-Zero must have at least 10 pb^{-1} of good data before a long (6-10 mo.) shutdown.

XI A History of Upgrades

A. Cornell

300 MeV	'49	
1 GeV	54	
2 GeV	64	
10 GeV	68	(SLAC 20 GeV Linac)
8x8 GeV	79	
8x8 GeV Upgrade	88	

B. BNL

AGS 30 GeV Upgrade (linac) '70 (Fermilab 200 GeV)
AGS Upgrade (booster etc) 88

C. SLAC

Linac	'67	
Spear	73	
PEP	79	
SLC	88	
400 GeV e^+e^- ?	92	

D. CERN

Cyclotron	'58	
PS	60	
ISR	71	
SPS	76	
$S\bar{p}pS$	81	
$S\bar{p}pS$ → ACOL	88	(TeV I Going)
LEP I	89	
LEP II	92	
LHC	?	

E. DESY

DORIS	'74	
PETRA	77	
DORIS Upgrade	85	
HERA	90	

F. FERMILAB

400 GeV	'72	
TeV I	87	
TeV II	84	
UPGRADE →	93	proposed

XII. Resume of Upgrade Virtues

1. Physics is first rate with very large discovery potential and strong programmatic power.

2. This is the highest energy machine in the world. It deserves the full exploitation compatible with realistic costs, time scale and manpower needs. It represents an investment of $500M in R&D, Equipment, construction and AIP funds. The history of upgrades also speaks eloquently to this.

3. HEP must maintain its excitement and its vitalilty, especially during the long construction schedule for the SSC. Discoveries, press releases, etc., will serve to keep the flow of new students and will insure the attention which is needed to secure a decent SSC funding profile.

4. The learning curve of new physics and of handling collider subtleties alone will pay the upgrade costs. These can modulate SSC detector design and will be relevant up to turn-on and beyond. CDF and D-Zero must learn to cope with subtle signatures at the level of 10^{-10} of the total cross-section. No amount of simulation substitutes for learning by doing. This acquired skill becomes the experience base of the SSC and is terribly cost effective at SSC annual costs of $250M/year. CDF and D-Zero at $> 10^{31}$ luminosity are unique sources of this learning curve.

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* Jan '82 Subpanel on Long Range Planning - Excerpts (p.29) "The achievement of a luminosity greater than $10^{30}cm^{-2}sec^{-1}$ will, in our judgement, take some years of operational experience., On the other hand, a number of improvements seem possible. Thus, an ultimate goal of $L = 10^{31}$ appears reasonable to us."

"The TEVATRON projects will be the focus of a major part of the U.S. program ... they will open up entirely new areas of physics and accelerator development and will be essentially unique in the world."

July '83 Subpanel on New Facilities (p.51) "The viability of the [TEVATRON] facility after about 1992 will depend on the physics interest and the availability of other facilities. If the level of research activity remains high, then an upgrade of the facility and its detectors may be warranted, with a consequent extension of the useful life of the machine for perhaps another five years."

Sept 85 Report of the 1985 HEP Study (p.27) "Because new phenomena may not conform to our current expectations, it is natural to expect the configuration of these detectors [CDF-D0] to evolve in response to our growing understanding ... A program of detector upgrades and accelerator improvements will be an essential part of the hadron collider physics program."

In fixed-target experiments ... experiments can be grouped in terms of the physics questions ...

(1) CP violation in Kaon Decays
(2) Rare Kaon Decays
(3) Heavy Quark Physics
(4) Hadron Dynamics Other than Perturbative QCD
(5) Neutrino Oscillation Experiments
(6) Particle Searches with Beam Dump

Chart I

Numbers of Produced Events in a 5 Mo (3×10^6 sec) Year Based on Assumed $\mathscr{L}p$

$\mathscr{L}p$ →	No Upgrade	Phase I	Phase II		
	3×10^{29}	4×10^{30}	4×10^{31} A.	2×10^{32} B.	1×10^{31} C.
CM Energy	2 TeV	2 TeV	2 TeV	2 TeV	$\geq$ 3 TeV
Int $\mathscr{L}$	$\bar{p}p$ $1pb^{-1}$	$\bar{p}p$ $10pb^{-1}$	$\bar{p}p$ $100pb^{-1}$	pp $500pb^{-1}$	$\bar{p}p$ $30pb^{-1}$
Mass	Discovery Limits			"Factory" Regime	
			Z^o		
200	500	5000	50 K	100 K	20 K
400	30	400	4 K	3.5 K	2.4 K
600	–	40	400	200	300
800	–	7	70	15	75
1000	–	–	10	–	30
			TOP		
75	300	3000	30 K	150 K	30 K
100	70	700	7 K	25 K	6 K
125	20	200	2 K	5 K	1.8 K
150	8	80	900	2 K	900
175	3	30	300	500	380
200	1	10	100	250	150
250	–	–	–	25	–
			Gluino		
100	300	3000	30 K	150 K	30 K
150	20	200	2000	10 K	3 K
200	2	20	200	800	500
250	–	3	30	100	90
300	–	–	5	10	30
			W-Pairs		
W^+W^-	5	50	500	1000	240
Z^oZ^o	–	7	70	150	40
W^+Z^o	0.7	7	70	200	30
			Technipions		
50	800	8000	80 K		62 K
100	200	2000	20 K		18 K
150	70	700	7 K		6.5 K
200	30	300	3 K		3 K
250	12	120	1.2K		1.4 K

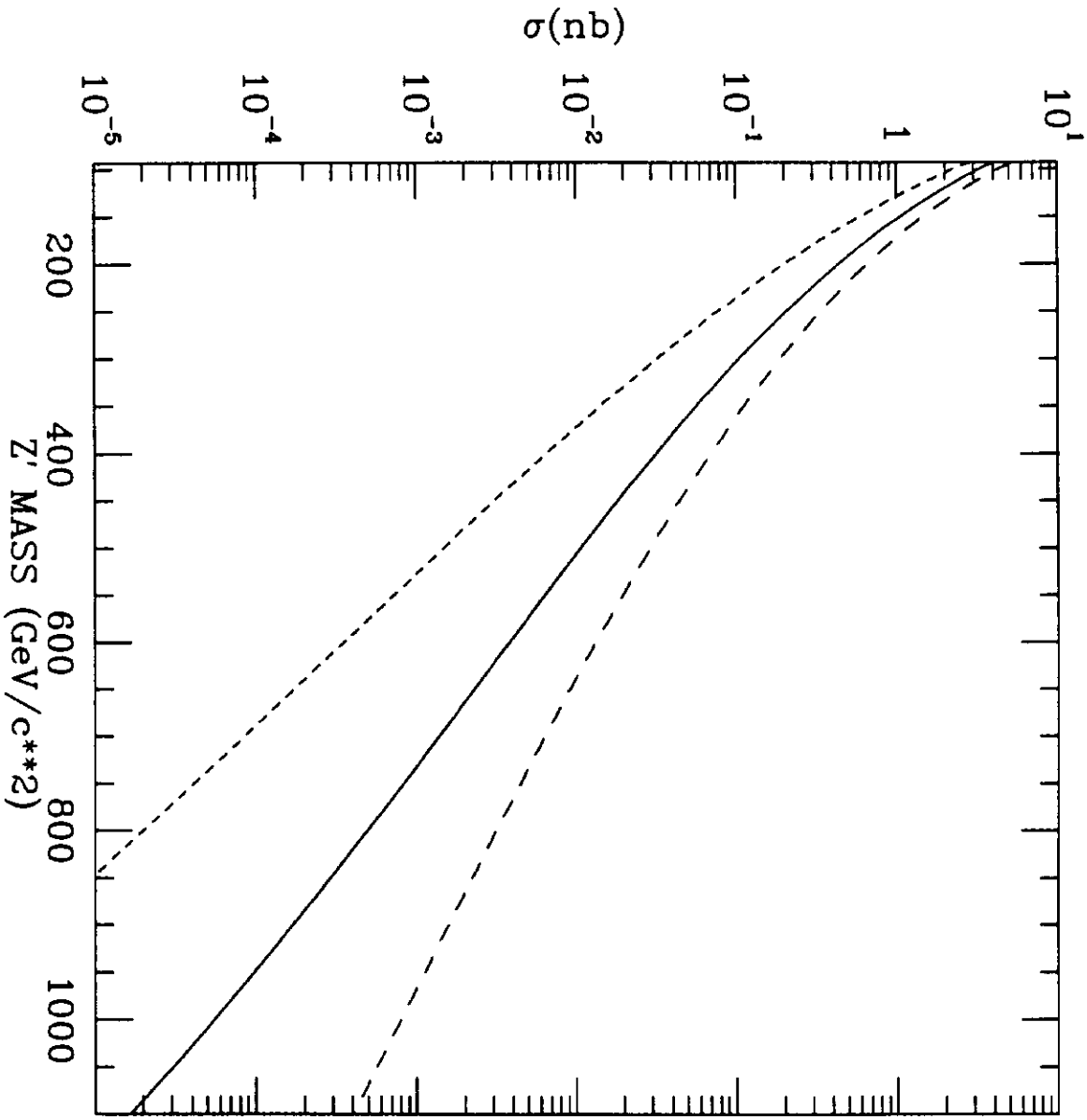

Fig. 1 Production of a heavy Z°
The mass reach is determined by the integrated luminosity and the discovery level required, e.g., 100 produced events. See Chart I for results.

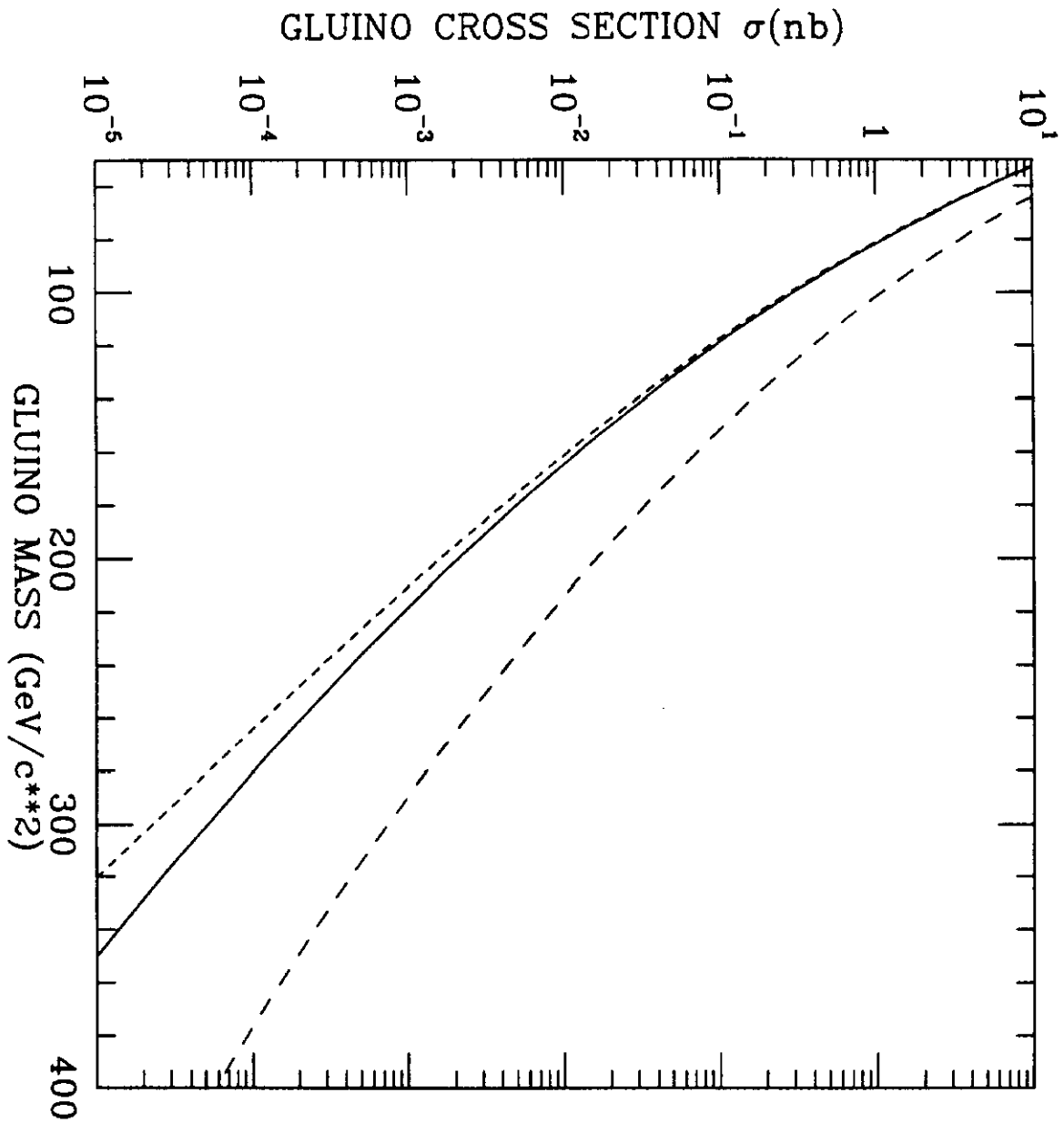

Fig. 2 Production of Gluinos

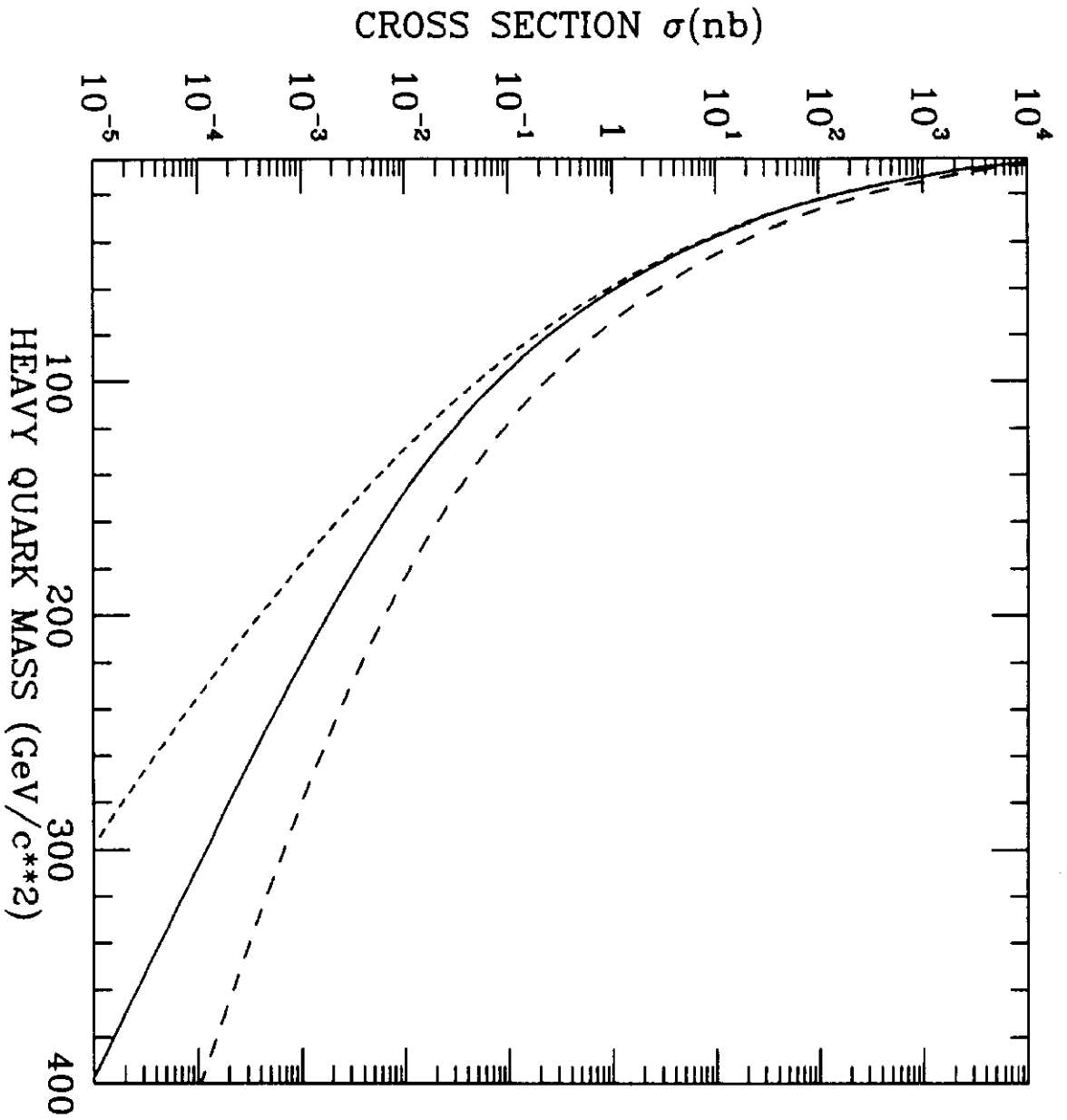

Fig. 3 Production of Heavy Quarks