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Course: SEAS 8414 — Security Analytics

Goal of this notebook

Train and audit detectors on the CSE-CIC-IDS2018 denial-of-service day, then grade the evidence behind the score.

What you will learn

1. Read a majority-class baseline before trusting any accuracy figure.
2. Find the strongest single feature, then test it by dropping it and refitting.
3. Tell duplicate inflation apart from genuine signal.
4. Report per-group recall, because the rare classes carry the risk.

Where this connects to the course text

The text builds a defence pipeline; this notebook trains a classifier and audits it. The links below are to specific chapter objectives that share an *analytic move*, not to matching subject matter.

- **Chapter 3: Vulnerability Assessment** — Learning objective 1 (section 3.1) frames assessment as **evidence grading**, not output collection. The A-F data-trust grade in section 10 is exactly that move, applied to a model score.
- **Chapter 11: Formal Protocol Verification** — Section **11.1.2**, titled *Proved, tested, and hoped*, asks you to separate exactly those three. (Chapter 11 lists its objectives in §11.0, not §11.1 as the other chapters do.) The ablation does that job here: it tests whether the headline survives.

CIC-IDS2018 Case Study — DoS (GoldenEye / Slowloris)

Model comparison + validity audit on the dos (goldeneye / slowloris) day of CSE-CIC-IDS2018

Abstract: We study one day of the CSE-CIC-IDS2018 flow corpus. Its attack traffic is **dos (goldeneye / slowloris)**. Benign flows still dominate the day numerically: the loader prints 1,048,575 flows at an attack rate of 0.0501. The four learners **train** on a 120,000-row stratified subsample of the training half. But they are **scored on the whole 262,144-row held-out split**. So the accuracy and ROC-AUC figures below are full-holdout

measurements of a subsample-*trained* model, not scores estimated on 120,000 rows. The question is whether the near-perfect in-distribution scores reflect detection, or the CICFlowMeter defects that Engelen et al. (2021) documented on **CICIDS2017**. The feature extractor is shared with this 2018 capture. Their corrections to individual flow labels are not shared with this capture, and nothing here re-audits the 2018 labels.

1. Research problem

Task: Detect DoS-GoldenEye and DoS-Slowloris connections on the 2018-02-15 capture. These low-and-slow / flooding attacks perturb flow-timing features. We do **not** run a controlled volume-versus-timing experiment here. We report which features the model leans on and whether the score survives dropping the strongest one. We also ask whether models learn timing signatures or shortcut on volume.

2. Literature review

- **Sharafaldin, Lashkari & Ghorbani (2018)** — the **CIC-IDS2017** dataset paper (ICISSP 2018, 108–116): B-Profile-generated benign traffic, a labelled attack schedule, and the 80-column CICFlowMeter feature set. CIC asks users of CSE-CIC-IDS2018 to cite it, but it is **not** a description of this 2018 capture. CSE-CIC-IDS2018 is a separate CSE–CIC collaboration run on AWS. It inherits the generation methodology and the feature extractor. It does not inherit the network, the hosts, the schedule or the labels.
- **Engelen, Rimmer & Joosen (2021)** — *Troubleshooting an Intrusion Detection Dataset: the CICIDS2017 Case Study* (IEEE S&P Workshops). The paper found labelling errors and CICFlowMeter implementation bugs that inflate scores. **Read the scope:** the audit is of **CICIDS2017**. The extractor bugs are a reasonable suspicion here, because the 2018 CSVs were produced with the same CICFlowMeter. The specific mislabelled flows they corrected do **not** transfer to this day. Nothing below re-audits the 2018 labels.
- **Rosay, Cheval, Carlier & Leroux (2022)** — *Network Intrusion Detection: A Comprehensive Analysis of CIC-IDS2017* (ICISSP): documents CICFlowMeter implementation flaws in these flow features. The paper again measured these flaws on the 2017 corpus, so treat the *mechanism* as transferable and the *measurements* as not.
- **Sommer & Paxson (2010)** — closed-world ML scores rarely survive deployment.
- **Apruzzese et al. (2023)** — *The Role of Machine Learning in Cybersecurity* (ACM DTRAP): a survey of where ML is and is not actually deployed in security practice. It is cited for that framing, not as a study of dataset shortcuts.

Representative approaches and their known caveats — drawn from the wider literature and qualitative only. These are **not** measurements reproduced on this corpus, so no

figures are quoted.

Reported approach	Known caveat
Sharafaldin et al. (2018) — classifier baselines on CIC-IDS**2017**	in-distribution; CICFlowMeter features later shown buggy; a different capture from this day
Engelen et al. (2021) — re-labelled CICIDS2017	original 2017 labels and features partly wrong; no equivalent re-labelling exists for 2018
Typical DL-NIDS papers	benign-majority base rate inflates accuracy; per-family recall varies

3. Dataset provenance & honesty caveats

Property	Value
Source	CSE-CIC-IDS2018, AWS Open Data `s3://cse-cic-ids2018/` (no credentials)
File	`Thursday-15-02-2018_TrafficForML_CICFlowMeter.csv`
Rows	**1,048,575** flow records, as printed by the loader. The download caps the stream at 1.2 M lines; this day's CSV is smaller than the cap, so the cap does not bind.
Features	80 CICFlowMeter columns in the file → **68 used** after dropping label/ID and constant columns (the loader prints the exact count)
Label	`Benign` vs the day's attack families (`DoS attacks-GoldenEye` , `DoS attacks-Slowloris`)
Attack rate	**0.0501**, printed by the loader

Honestly: CICFlowMeter's feature implementation has documented bugs, but the documentation is of **CICIDS2017** (Engelen et al. 2021; Rosay et al. 2022), not of this 2018 day. The extractor is the same, so the feature-level defects are a live suspicion here; the label corrections those authors published are 2017-specific and are **not** applied below. Accuracy is separately inflated by the benign-majority base rate. We report per-attack-family recall and audit single-feature shortcuts for those reasons.

A shortcut deliberately named, not removed — `Dst Port` : The loader's drop list is only `Label` , `Timestamp` , `y` and `family` . So `Dst Port` and `Protocol` survive into `X` . Both of this day's tools are HTTP denial-of-service attacks aimed at a single victim host. CIC's published 2018-02-15 schedule runs GoldenEye 09:26–10:09 and Slowloris 10:59–11:40 against the same target. Benign traffic on the same day spreads across DNS, HTTPS, RDP, SMB and more. Destination port therefore acts as a cheap **negative-class filter**. It encodes the capture schedule rather than attack behaviour. A model can discard a large share of benign flows without learning anything about denial of service. This notebook does **not** quantify that effect. Nor is `Dst Port` the shortcut the section-10 audit surfaces (it prints `Fwd Seg Size Min` as the strongest single

feature). But the column is in the matrix, and a deployment-realistic rerun would drop it before any of the numbers below are read.

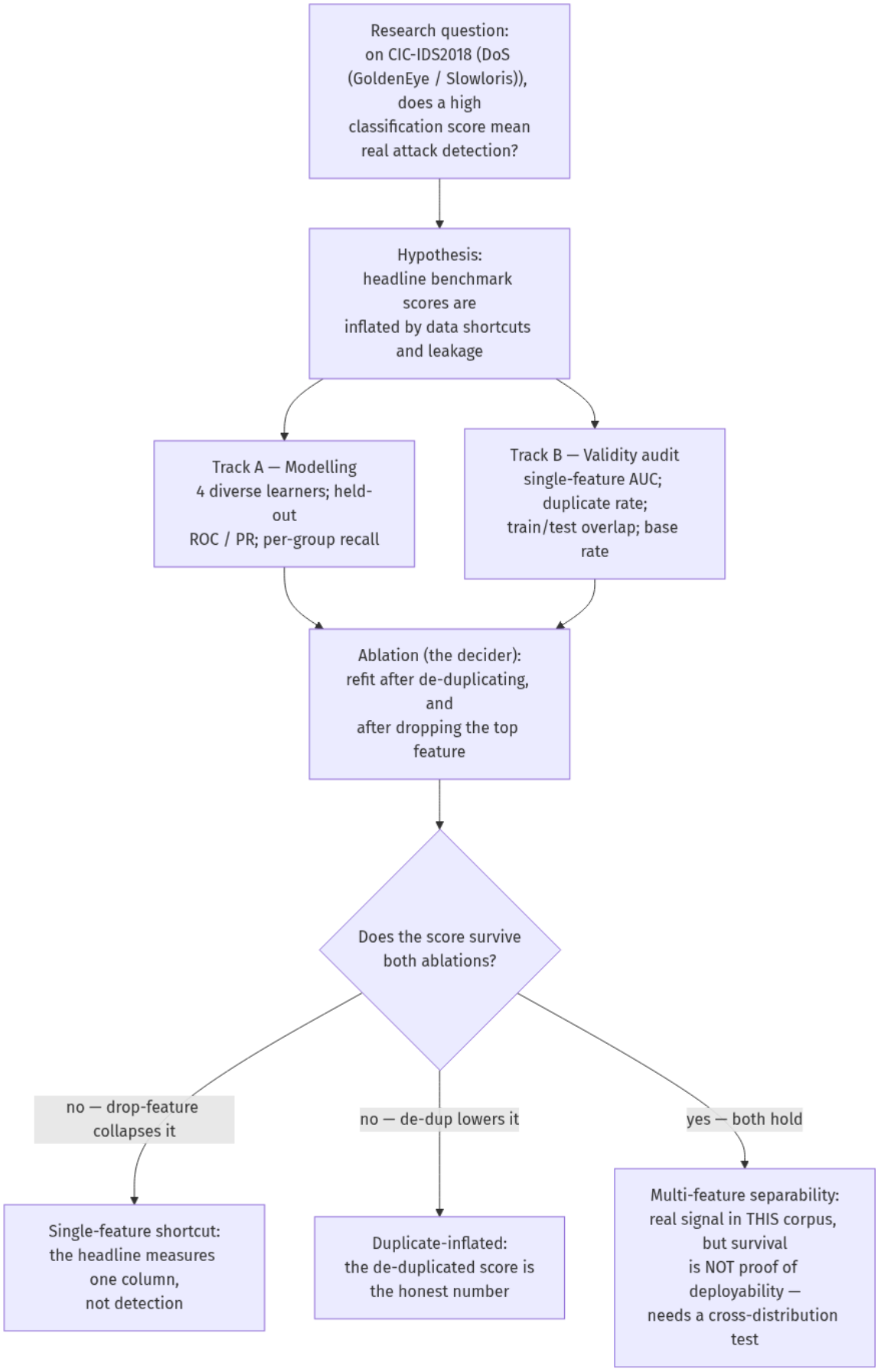
Before you run this: getting the data

This notebook downloads its own data on the first run, then caches it. **No Kaggle account and no credentials are needed** - the source is the public AWS Open Data bucket `s3://cse-cic-ids2018/`.

4. Solution design

The methodology is deliberately two-track. We *earn* a headline score with standard modelling, then *interrogate* it with a validity audit. Only a verdict that survives both is reported. The diagram below is the shape of every notebook in this series.

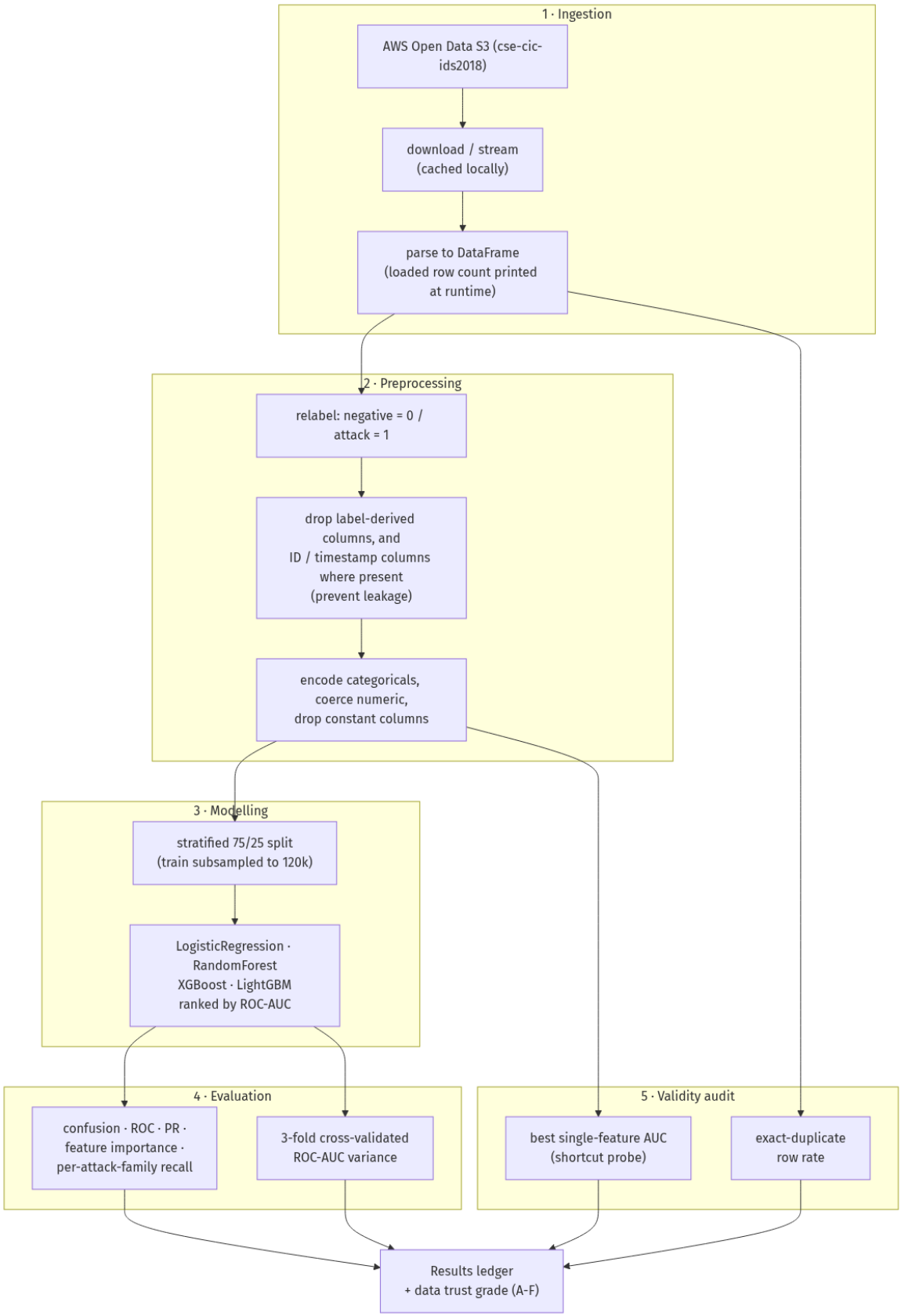
Figure 4.1 — Solution design (methodology).



5. Implementation architecture

Five stages — ingestion, preprocessing, modelling, evaluation, and a parallel validity-audit path — feed a single graded results ledger. Leakage defences (dropping label-derived and identifier columns) live in preprocessing, before any model sees the data.

Figure 5.1 — Implementation architecture.



6. Data acquisition & preparation

Every line below is commented so a student can re-run and modify each step. The cell ends by producing the standard analysis variables: `df`, `X` (clean numeric features), `y` (binary label), `feat` (feature names), and `family`. `family` is the per-group label used for the recall breakdown. It is an attack family on the intrusion corpora, but a transaction type, merchant category or malware category on the fraud/malware ones.

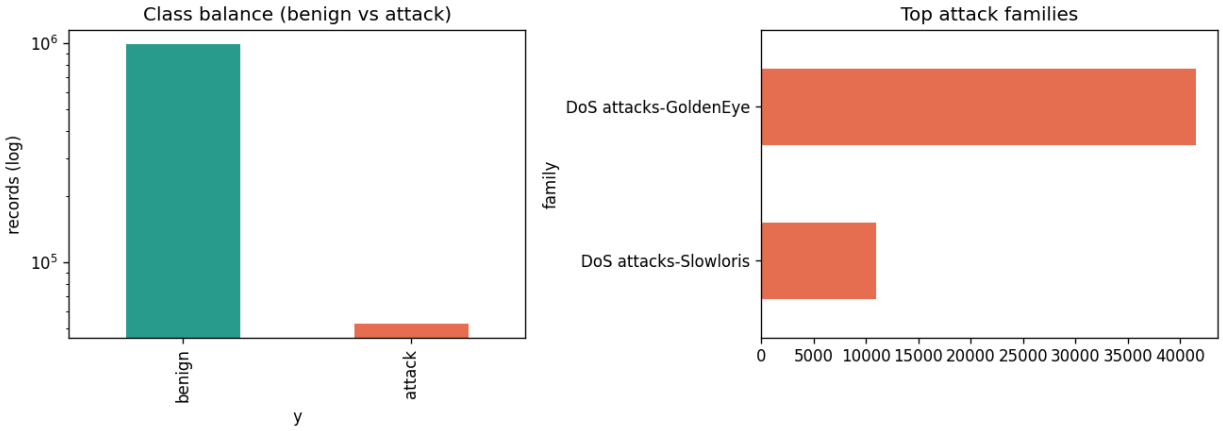
In [1]:
```
%matplotlib inline
import time, warnings; warnings.filterwarnings('ignore')   # keep output clean
import numpy as np, pandas as pd                            # numerics + dataframes
import matplotlib.pyplot as plt                             # static plots (embed in HTML+PDF)
plt.rcParams['figure.dpi'] = 120                            # crisp figures
RANDOM_STATE = 0                                            # single seed used everywhere
np.random.seed(RANDOM_STATE)                                # reproducible sampling
NEG_WORD, POS_WORD = 'benign', 'attack'                     # class names (overridden by some loaders)
```

In [2]:
```
import os
FILE = 'Thursday-15-02-2018_TrafficForML_CICFlowMeter.csv'; SHORT = 'dos'
PREFIX = 's3://cse-cic-ids2018/Processed Traffic Data for ML Algorithms/'
CACHE = f'/tmp/cic_{SHORT}.csv'
if not os.path.exists(CACHE):                               # bounded S3 download, no credentials
    os.system(f'aws s3 cp "{PREFIX}{FILE}" - --no-sign-request 2>/dev/null | head -n 1200000 > "{CACHE}"')
df = pd.read_csv(CACHE, low_memory=False)                   # parse the day's flow records
df = df[pd.to_numeric(df['Dst Port'], errors='coerce').notna()].reset_index(drop=True)  # drop repeated-header junk
df['Label'] = df['Label'].astype(str).str.strip()           # clean labels
df['y'] = (df['Label'] != 'Benign').astype(int)             # 1 = attack, 0 = benign
df['family'] = df['Label']                                  # attack type doubles as family
assert len(df) >= 1_000_000, f'floor not met: {len(df):,}'  # honesty gate: >= 1M rows
DROP = ['Label','Timestamp','y','family']
feat = [c for c in df.columns if c not in DROP]
X = df[feat].apply(pd.to_numeric, errors='coerce').replace([np.inf,-np.inf],np.nan).fillna(0.0)
X = X.loc[:, X.nunique() > 1]; feat = list(X.columns)       # drop constants; align feat
y = df['y'].to_numpy()                                      # STANDARD CONTRACT: binary label
print(f'loaded {len(df):,} flows x {len(feat)} features; attack rate {y.mean():.4f}')
```

```
loaded 1,048,575 flows x 68 features; attack rate 0.0501
```

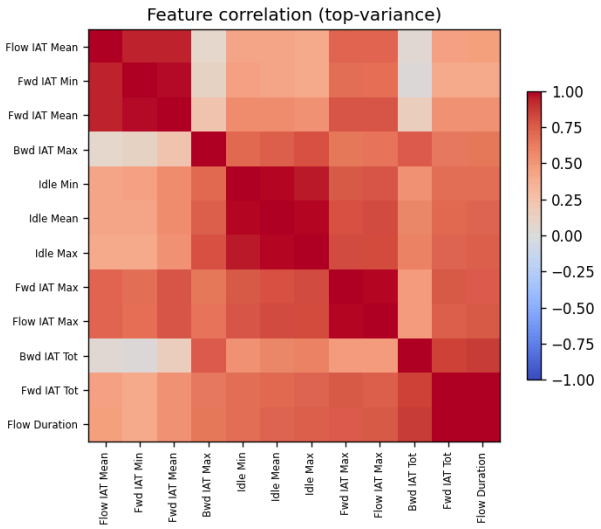
7. Exploratory data analysis

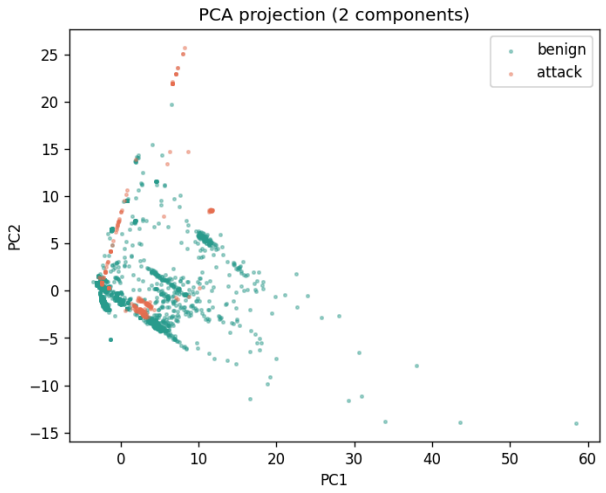
In [3]:
```
# --- EDA 1: class balance and the attack-family mix ---
fig, ax = plt.subplots(1, 2, figsize=(11, 4))
df['y'].map({0:NEG_WORD,1:POS_WORD}).value_counts().plot.bar(
# counts per class
    ax=ax[0], color=['#2a9d8f','#e76f51']); ax[0].set_yscale('log')
ax[0].set_title(f'Class balance ({NEG_WORD} vs {POS_WORD})');
ax[0].set_ylabel('records (log)')
df.loc[df.y==1,'family'].value_counts().head(8).plot.barh(
# top attack families
    ax=ax[1], color='#e76f51'); ax[1].invert_yaxis(); ax[1].set_title('Top
attack families')
plt.tight_layout(); plt.show()
```



In [4]:
```
# --- EDA 2: feature correlation + a 2-D PCA projection ---
from sklearn.preprocessing import StandardScaler                    # scale
before PCA
from sklearn.decomposition import PCA
fig, ax = plt.subplots(1, 2, figsize=(12, 5))
topv = X[feat].var().sort_values().tail(12).index                   # 12
highest-variance features
im = ax[0].imshow(X[topv].corr(), cmap='coolwarm', vmin=-1, vmax=1)  #
correlation heatmap
ax[0].set_xticks(range(len(topv))); ax[0].set_xticklabels(topv,
rotation=90, fontsize=7)
ax[0].set_yticks(range(len(topv))); ax[0].set_yticklabels(topv, fontsize=7)
ax[0].set_title('Feature correlation (top-variance)'); fig.colorbar(im,
ax=ax[0], shrink=0.7)
samp = X.sample(min(5000, len(X)), random_state=RANDOM_STATE)     #
subsample for a fast PCA
pc =
PCA(n_components=2).fit_transform(StandardScaler().fit_transform(samp))
ys = y[samp.index]                                                  # aligned
labels for coloring
for lab,c in [(0,'#2a9d8f'),(1,'#e76f51')]:
    ax[1].scatter(pc[ys==lab,0], pc[ys==lab,1], s=4, alpha=0.4, color=c,
                  label={0:NEG_WORD,1:POS_WORD}[lab])
ax[1].set_title('PCA projection (2 components)'); ax[1].legend();
```

```
ax[1].set_xlabel('PC1'); ax[1].set_ylabel('PC2')
plt.tight_layout(); plt.show()
```





8. Model comparison

Four diverse learners share one held-out split, ranked by ROC-AUC.

Two honesty guards print with the table:

1. The models **train** on a *stratified subsample* of at most 120,000 rows, drawn from the training half of a 75/25 stratified split. They are then **scored on the entire held-out half**. The cell prints both counts. The held-out half (262,144 rows) is more than twice the size of the training subsample. So the figures below are full-holdout numbers from a subsample-*trained* model: the training set is bounded, the evaluation set is not. (The inline comment in the cell calls them "SUBSAMPLE" numbers; read that as subsample-**trained**. What the cap limits is how much the learners could fit, not how many rows the score was measured on.)
2. The **majority-class baseline accuracy** appears *inside* the ranking table. On imbalanced data, 0.99 accuracy can be worse than always guessing the majority class. Judge each model against that baseline, not against 0.5.

In [5]:
```
# --- Model comparison: four learners on the same held-out split ---
from sklearn.model_selection import train_test_split
from sklearn.linear_model import LogisticRegression
from sklearn.ensemble import RandomForestClassifier
from sklearn.metrics import accuracy_score, roc_auc_score
import xgboost as xgb, lightgbm as lgb

# Stratified split keeps the class ratio in both halves.
Xtr, Xte, ytr, yte = train_test_split(X, y, test_size=0.25,
random_state=RANDOM_STATE, stratify=y)
from sklearn.pipeline import make_pipeline
from sklearn.preprocessing import StandardScaler
N_MATERIALIZED = len(y)                                    # the full
corpus we loaded (see printed count)
```

```
# HONEST DISCLOSURE: we do NOT train on all N. We fit on a STRATIFIED
subsample (<=120k) because
# these learners saturate long before then on this data. Every headline
below is a SUBSAMPLE
# number, not a full-corpus number — saying otherwise would be the
fabrication this course forbids.
if len(Xtr) > 120_000:
    Xtr, _, ytr, _ = train_test_split(Xtr, ytr, train_size=120_000,
random_state=RANDOM_STATE,
                                      stratify=ytr)               #
genuinely stratified, not random
MAJORITY_BASELINE = max(np.mean(yte), 1 - np.mean(yte))          # accuracy
of 'always predict majority'
print(f'materialized {N_MATERIALIZED:,} rows | trained on {len(Xtr):,}
(stratified subsample) | '
      f'held-out {len(yte):,}')
print(f'MAJORITY-CLASS BASELINE accuracy = {MAJORITY_BASELINE:.4f}  '
      f'(any model must beat THIS, not 0.5, to be interesting)')

models = {                                                        # four
standard, diverse learners
    'LogisticRegression': make_pipeline(StandardScaler(),
LogisticRegression(max_iter=300)),  # scaled!
    'RandomForest': RandomForestClassifier(n_estimators=60, n_jobs=-1,
random_state=RANDOM_STATE),
    'XGBoost': xgb.XGBClassifier(n_estimators=80, max_depth=6,
tree_method='hist', n_jobs=-1,
                                 eval_metric='logloss',
random_state=RANDOM_STATE),
    'LightGBM': lgb.LGBMClassifier(n_estimators=80, n_jobs=-1, verbose=-1,
random_state=RANDOM_STATE),
}
rows, fitted = [], {}
for name, m in models.items():                                    # fit +
score each model
    t = time.perf_counter(); m.fit(Xtr, ytr); fitted[name] = m
    p = m.predict_proba(Xte)[:, 1]                                #
positive-class probability on held-out
    rows.append({'model': name, 'accuracy': round(accuracy_score(yte,
(p>0.5).astype(int)), 6),
                 'roc_auc': round(roc_auc_score(yte, p), 6),      # 6 dp: a
1.000000 is a red flag, not a win
                 'train_s': round(time.perf_counter()-t, 1)})
rows.append({'model': 'MajorityBaseline', 'accuracy':
round(MAJORITY_BASELINE, 4),
             'roc_auc': 0.5, 'train_s': 0.0})            # show the
baseline IN the ranking table
comparison = pd.DataFrame(rows).sort_values('roc_auc',
ascending=False).reset_index(drop=True)
_ranked = comparison[comparison.model != 'MajorityBaseline']
best_name = _ranked.iloc[0]['model']; best = fitted[best_name]  # winner by
ROC-AUC (excl. baseline)
print('best model:', best_name); comparison
```

```
materialized 1,048,575 rows | trained on 120,000 (stratified subsample) |
held-out 262,144
MAJORITY-CLASS BASELINE accuracy = 0.9499  (any model must beat THIS, not
0.5, to be interesting)
best model: RandomForest
```

Out[5]:

	model	accuracy	roc_auc	train_s
0	RandomForest	0.999908	1.000000	0.8
1	XGBoost	0.999939	0.999999	0.4
2	LightGBM	0.999939	0.999992	1.1
3	LogisticRegression	0.999416	0.999925	0.3
4	MajorityBaseline	0.949900	0.500000	0.0

9. Results

Diagnostics for the winning model, including **per-group recall**.

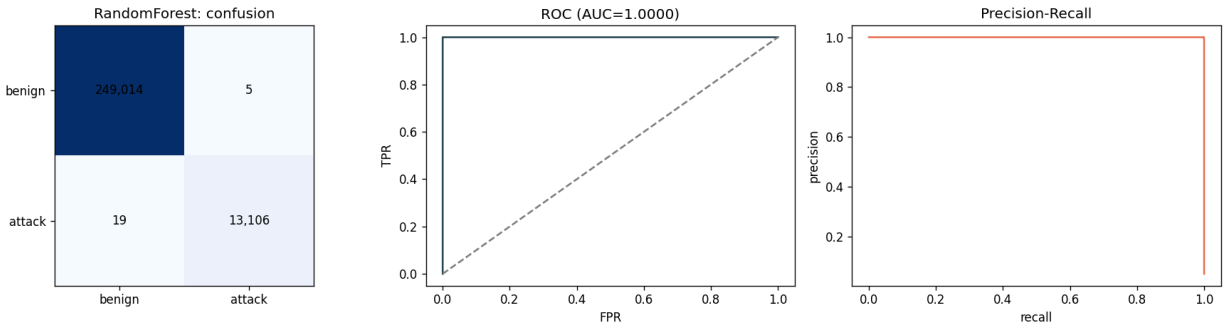
The grouping comes from whatever the loader put in `family`. It is *not* always an attack taxonomy. On the intrusion corpora it is the attack family. On the fraud and malware corpora it is a transaction type, a merchant category or a malware category. On binary corpora it collapses to the positive class.

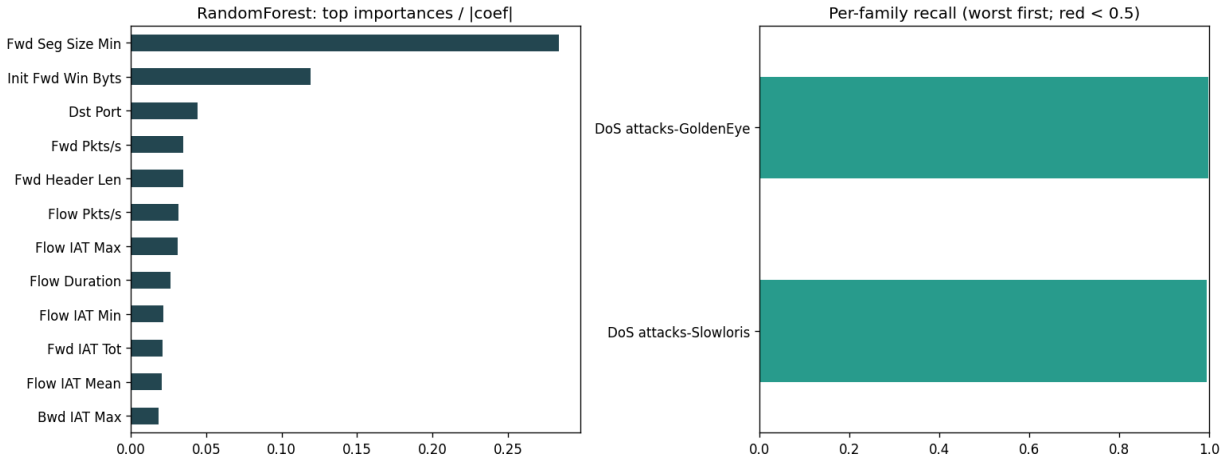
Read it accordingly. Where the groups are genuinely rare classes, they reveal whether detection is real. The dominant flood classes do not.

In [6]:

```
# --- Results for the best model: confusion, ROC, PR, importances, per-
family recall ---
from sklearn.metrics import confusion_matrix, roc_curve,
precision_recall_curve, recall_score
pb = best.predict_proba(Xte)[:, 1]; pred = (pb > 0.5).astype(int)
fig, ax = plt.subplots(1, 3, figsize=(15, 4))
# (1) confusion matrix
cm = confusion_matrix(yte, pred); ax[0].imshow(cm, cmap='Blues')
ax[0].set_title(f'{best_name}: confusion'); ax[0].set_xticks([0,1]);
ax[0].set_yticks([0,1])
ax[0].set_xticklabels([NEG_WORD,POS_WORD]);
ax[0].set_yticklabels([NEG_WORD,POS_WORD])
for (i,j),v in np.ndenumerate(cm):
ax[0].text(j,i,f'{v:,}',ha='center',va='center')
# (2) ROC and PR curves
fpr,tpr,_ = roc_curve(yte, pb); prec,rec,_ = precision_recall_curve(yte,
pb)
ax[1].plot(fpr,tpr,color='#264653'); ax[1].plot([0,1],[0,1],'--',c='grey')
ax[1].set_title(f'ROC (AUC={roc_auc_score(yte,pb):.4f})');
ax[1].set_xlabel('FPR'); ax[1].set_ylabel('TPR')
ax[2].plot(rec,prec,color='#e76f51'); ax[2].set_title('Precision-Recall');
ax[2].set_xlabel('recall'); ax[2].set_ylabel('precision')
plt.tight_layout(); plt.show()
```

```
# (3) feature importances + (4) per-attack-family recall
fig, ax = plt.subplots(1, 2, figsize=(13, 5))
imp, names = None, feat                                                 #
importances, robust to the scaled-LR pipeline
if hasattr(best, 'feature_importances_'):                                # tree
models
    imp = best.feature_importances_; names = list(getattr(best,
'feature_names_in_', feat))[:len(imp)]
elif hasattr(best, 'named_steps') and 'logisticregression' in getattr(best,
'named_steps', {}):
    imp = np.abs(best.named_steps['logisticregression'].coef_[0]); names =
feat  # LR pipeline
elif hasattr(best, 'coef_'):
    imp = np.abs(best.coef_[0]); names = feat
if imp is not None:
    pd.Series(imp,
index=names[:len(imp)]).sort_values().tail(12).plot.barh(ax=ax[0],
color='#264653')
ax[0].set_title(f'{best_name}: top importances / |coef|')
# Per-family recall, WORST-first so rare, hard classes are visible, not
just the dominant floods.
fam_te = df.loc[Xte.index, 'family']
fr = {}
for fam, cnt in fam_te[yte==1].value_counts().items():
    if cnt < 5: continue                                                # need a
few positives for a meaningful recall
    mask = (fam_te==fam).to_numpy(); fr[fam] = recall_score(yte[mask],
pred[mask], zero_division=0)
srt = pd.Series(fr).sort_values()
show = pd.concat([srt.head(9), srt.tail(3)]) if len(srt) > 12 else srt   #
worst 9 + best 3
show = show[~show.index.duplicated()]
show.plot.barh(ax=ax[1], color=['#e76f51' if v < 0.5 else '#2a9d8f' for v
in show]); ax[1].set_xlim(0,1)
ax[1].set_title('Per-family recall (worst first; red < 0.5)')
plt.tight_layout(); plt.show()
# Operational numbers, not just figures: false-positive rate and the worst
per-family recalls.
tn, fp = int(cm[0,0]), int(cm[0,1])
fpr_op = fp/(fp+tn) if (fp+tn) > 0 else float('nan')                    # benign
wrongly flagged @0.5
print(f'operational FALSE-POSITIVE RATE @0.5 = {fpr_op:.4f}  ({fp:,} benign
flagged of {fp+tn:,})')
print('worst per-family recalls:', {k: round(v, 3) for k, v in
srt.head(6).items()})
```






```
operational FALSE-POSITIVE RATE @0.5 = 0.0000  (5 benign flagged of 249,019)
worst per-family recalls: {'DoS attacks-Slowloris': 0.996, 'DoS attacks-
GoldenEye': 0.999}
```

10. Validity audit — is the score real?

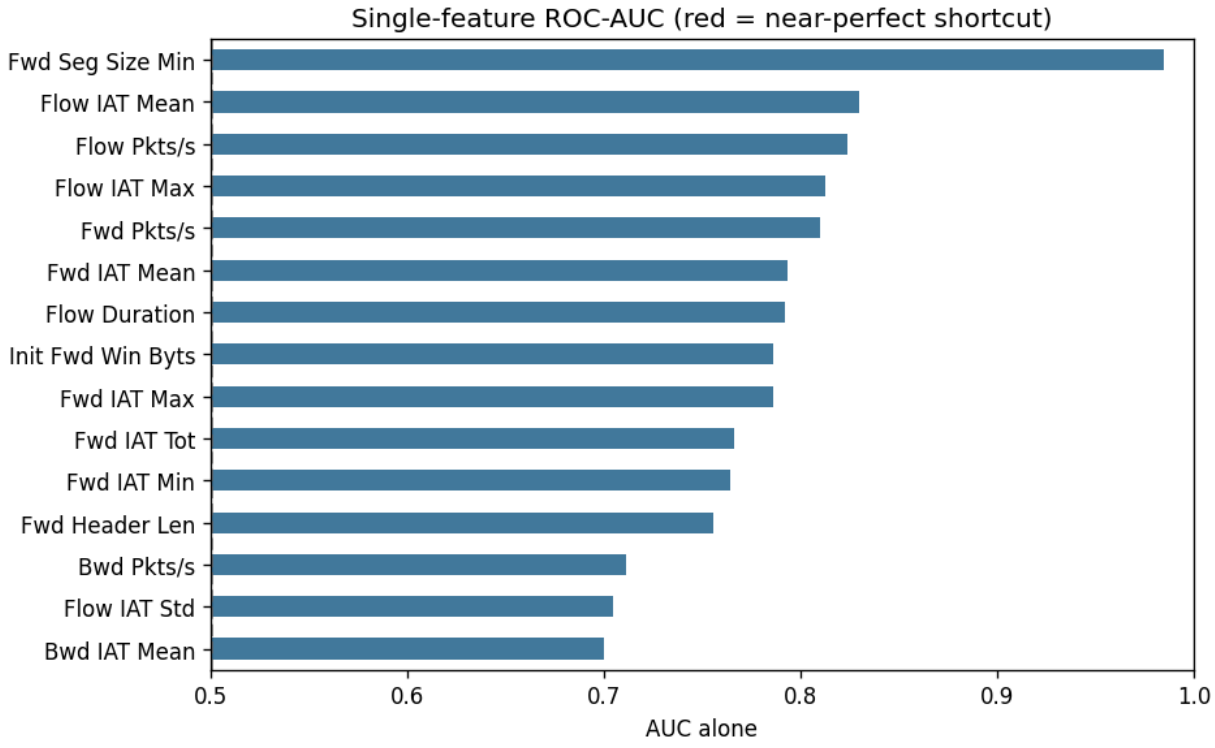
Three diagnostics. **(a)** How well can the *single best feature*, alone, separate the classes? A near-1.0 single-feature AUC means that feature is *near-sufficient* — a shortcut (which may be legitimate signal or an artifact), not the same as target leakage. **(b)** The exact-duplicate row rate. **(c)** The **train/test exact-row contamination** — the fraction of held-out rows that are duplicates of training rows, which is what actually inflates a held-out score. The trust grade is the *worse* of the single-feature and contamination concerns.

In [7]:

```
# --- Validity audit: is the score real detection, or a data shortcut? ---
from sklearn.metrics import roc_auc_score
samp = X.sample(min(60_000, len(X)), random_state=1); ysamp = y[samp.index]
aucs = {}
for c in feat:                                                  # AUC of
EACH feature alone
    col = samp[c].to_numpy(float)
    if col.std()==0: continue
    a = roc_auc_score(ysamp, col); aucs[c] = max(a, 1-a)          #
direction-agnostic
best_auc = max(aucs.values()); best_col = max(aucs, key=aucs.get)
dup_rate = 1 - X.drop_duplicates().shape[0]/len(X)              # exact-
duplicate feature rows (whole set)
# The statistic that actually inflates a held-out score is TRAIN/TEST
CONTAMINATION: how many test
# rows are exact duplicates of a training row. Measure it directly on the
split used above.
_trkeys = set(map(tuple, np.round(Xtr.to_numpy(), 6)))
_te = np.round(Xte.to_numpy(), 6)[:50_000]
contam = float(np.mean([tuple(r) in _trkeys for r in _te]))      # fraction
of test rows seen in train
# Trust grade reflects BOTH failure modes and takes the WORSE of the two: a
near-perfect single
# feature (shortcut) OR heavy train/test contamination each independently
invalidate the headline.
_ga = 'F' if best_auc>=0.999 else 'D' if best_auc>=0.99 else 'C' if
```

```
best_auc>=0.95 else 'B' if best_auc>=0.85 else 'A'
_gc = 'F' if contam>=0.5 else 'D' if contam>=0.3 else 'C' if contam>=0.15
else 'B' if contam>=0.05 else 'A'
grade = max(_ga, _gc)                                          # 'max'
letter = worse grade (A best, F worst)
print(f'best single-feature AUC = {best_auc:.4f}  (feature: {best_col})')
print(f'   note: a near-1.0 single-feature AUC means this feature is *near-
sufficient* (a shortcut),\n'
      f'   which may be legitimate signal OR an artifact — it is NOT the
same as target leakage.')
print(f'exact-duplicate row rate (whole corpus) = {dup_rate:.3f}')
print(f'TRAIN/TEST exact-row contamination       = {contam:.3f}  (single-
feat grade {_ga}, contam grade {_gc})')
print(f'==> data trust grade: {grade}   (worse of the two; F = shortcut
and/or heavy contamination)')
s = pd.Series(aucs).sort_values().tail(15)
fig, ax = plt.subplots(figsize=(8,5))
s.plot.barh(ax=ax, color=['#e76f51' if v>=0.99 else '#457b9d' for v in s]);
ax.axvline(0.5,ls='--',c='grey')
ax.set_xlim(0.5,1.0); ax.set_title('Single-feature ROC-AUC (red = near-
perfect shortcut)'); ax.set_xlabel('AUC alone')
plt.tight_layout(); plt.show()
```

```
best single-feature AUC = 0.9852  (feature: Fwd Seg Size Min)
   note: a near-1.0 single-feature AUC means this feature is *near-
sufficient* (a shortcut),
   which may be legitimate signal OR an artifact — it is NOT the same as
target leakage.
exact-duplicate row rate (whole corpus) = 0.156
TRAIN/TEST exact-row contamination       = 0.104  (single-feat grade C,
contam grade B)
==> data trust grade: C   (worse of the two; F = shortcut and/or heavy
contamination)
```



11. Ablation — does the headline survive removing the artifacts?

Narrating a shortcut is not enough. We *retrain the winning model* after (1) de-duplicating the corpus (removing the train/test contamination) and (2) dropping the single strongest feature. We report the held-out AUC each time. **Read the result honestly, both ways:** if the AUC **collapses**, the headline was a contamination/shortcut artifact. If it **barely moves** — common on *simulated* corpora — that is **not vindication**. It means the classes are separable by *many* redundant features because the attack and benign distributions barely overlap. That is its own generation artifact. The numbers below decide which story is true here, not the prose.

In [8]:
```
# --- Ablation: SHOW the inflation empirically, don't just narrate it ---
from sklearn.base import clone
def _retrain_auc(Xa, ya):                                          # re-split, stratified-subsample, refit best family
    xtr, xte, ytr2, yte2 = train_test_split(Xa, ya, test_size=0.25, random_state=RANDOM_STATE, stratify=ya)
    if len(xtr) > 120_000:
        xtr, _, ytr2, _ = train_test_split(xtr, ytr2, train_size=120_000, random_state=RANDOM_STATE, stratify=ytr2)
    m = clone(best); m.fit(xtr, ytr2)
    return roc_auc_score(yte2, m.predict_proba(xte)[:, 1])
base_auc = roc_auc_score(yte, best.predict_proba(Xte)[:, 1])     # (0) the headline held-out AUC
Xdd = X.drop_duplicates(); ydd = y[Xdd.index]                      # (1) de-duplicated corpus
auc_dedup = _retrain_auc(Xdd, ydd)
auc_noshort = _retrain_auc(X.drop(columns=[best_col]), y) if best_col in X.columns else base_auc  # (2) drop shortcut
ablation = pd.DataFrame([
    {'setting': 'headline (as-is)',              'held_out_auc': round(base_auc, 6)},
    {'setting': f'de-duplicated ({1-len(Xdd)/len(X):.0%} rows removed)', 'held_out_auc': round(auc_dedup, 6)},
    {'setting': f'shortcut feature dropped ({best_col})', 'held_out_auc': round(auc_noshort, 6)},
])
print('Ablation — how much of the headline survives once each artifact is removed:')
ablation
```

```
Ablation — how much of the headline survives once each artifact is removed:
```

Out[8]:

	setting	held_out_auc
0	headline (as-is)	1.000000
1	de-duplicated (16% rows removed)	1.000000
2	shortcut feature dropped (Fwd Seg Size Min)	0.999769

12. Reproducibility & robustness

In [9]:
```
# --- Reproducibility & robustness ---
import sklearn
from sklearn.model_selection import StratifiedKFold, cross_val_score
print(f'seed={RANDOM_STATE} | numpy {np.__version__} | sklearn
{sklearn.__version__} | '
      f'xgboost {xgb.__version__} | lightgbm {lgb.__version__}')
# 3-fold cross-validated ROC-AUC of the winning model (fresh clone, bounded
subsample) -> mean +/- std.
from sklearn.base import clone
cvX, cvy = Xtr.iloc[:40_000], ytr[:40_000]
def _auc_scorer(est, Xv, yv):                                    # robust to
xgboost's 2-col predict_proba
    p = est.predict_proba(Xv)
    p = p[:, 1] if getattr(p, 'ndim', 1) == 2 else p
    return roc_auc_score(yv, p)
try:
    cv = cross_val_score(clone(best), cvX, cvy,
                         cv=StratifiedKFold(3, shuffle=True,
random_state=RANDOM_STATE),
                         scoring=_auc_scorer, error_score='raise')
    assert np.all(np.isfinite(cv)), 'non-finite CV folds'   # FAIL CLOSED:
never narrate a NaN as evidence
    print(f'{best_name} 3-fold CV ROC-AUC = {cv.mean():.4f} +/-
{cv.std():.4f}  '
          f'(mean +/- std across 3 stratified folds; a small std means a
stable estimate on this split)')
except Exception as e:
    print(f'CV UNAVAILABLE ({type(e).__name__}: {str(e)[:60]}); rely on the
single held-out AUC above — '
          f'we do NOT report a CV number we could not compute')
```

```
seed=0 | numpy 2.3.5 | sklearn 1.9.0 | xgboost 1.6.2 | lightgbm 4.7.0
RandomForest 3-fold CV ROC-AUC = 1.0000 +/- 0.0000  (mean +/- std across 3
stratified folds; a small std means a stable estimate on this split)
```

13. Scientific conclusion

Per-family recall is high for both GoldenEye and Slowloris in-distribution — but that says the *capture* is separable, not that the detector generalizes.

Validity ledger — read the headline against these printed numbers: every score here was measured on the **full 262,144-row held-out split**. Only the *training* set was capped at a 120,000-row stratified subsample. Majority-class baseline **accuracy**: **0.9499**. The accuracy column must clear that bar to mean anything. For ROC-AUC the trivial baseline is 0.5, not that figure. Winning learner: **RandomForest** (3-fold CV ROC-AUC **1.0000**). Strongest *single* feature: `Fwd Seg Size Min` at AUC **0.9852**. The ablation refutes a single-feature story. Dropping that feature barely moves the AUC: **1.000000 → 0.999769**. So the separability is **multi-feature**. De-duplication does **not** lower the score

either (**1.000000**), so duplicate rows are not what props it up, even though **0.156** of the corpus rows are exact duplicates. Overlap is not heavy, but it is **not negligible either** (grade B). The random split still flatters the headline a little. Data-trust grade: **C**. It is the worse of two independent sub-checks. Single-feature AUC 0.9852 scores **C**. Train/test exact-row overlap 0.104 scores **B**. The single-feature check drives the grade, not the overlap check. Operational false-positive rate at threshold 0.5: **0.0000**. Worst per-group recalls, exactly as printed: { `DoS attacks-Slowloris` : 0.996, `DoS attacks-GoldenEye` : 0.999}. The weakest group sits at **0.996**, which is where detection is thinnest.

What the multi-feature result does and does not license: Many redundant columns separate the classes, so no single leaky column explains the score. That is a statement about how this corpus was generated — two attack tools, one victim, two narrow time windows, set against profiled synthetic benign traffic. This is not a clean bill of health. It is also **not** a reproduction of the CICFlowMeter bug findings: Engelen et al. (2021) and Rosay et al. (2022) audited **CICIDS2017**, and nothing above re-measures their defects, or their label corrections, on this 2018 day. The shared extractor is grounds for suspicion, not evidence.

A shortcut left in the matrix: `Dst Port` was never dropped. Both attacks are HTTP floods against one victim host. So the port column separates much of the benign traffic from the attack for free, with no denial-of-service behaviour learned. It is not the feature the single-feature audit ranks first. But it is a capture artifact sitting in `X`. For that reason alone, every number above should be read as an upper bound.

How the audit numbers are computed: overlap is measured on the first 50,000 held-out rows, so read it as a sampled estimate. Each ablation re-splits and refits, so tiny differences are re-split noise. The de-duplication variant keeps the first label when a feature vector appears twice. **Scope:** the split is random, not temporal or entity-grouped. Every number above therefore measures in-distribution separability only.

References

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2. Engelen, G., Rimmer, V. & Joosen, W. (2021). Troubleshooting an Intrusion Detection Dataset: the CICIDS2017 Case Study. *IEEE S&P Workshops*.
3. Rosay, A., Cheval, E., Carlier, F. & Leroux, P. (2022). Network Intrusion Detection: A Comprehensive Analysis of CIC-IDS2017. *8th Int. Conf. on Information Systems Security and Privacy (ICISSP)*, 25–36.

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